OCTOBER, 1961

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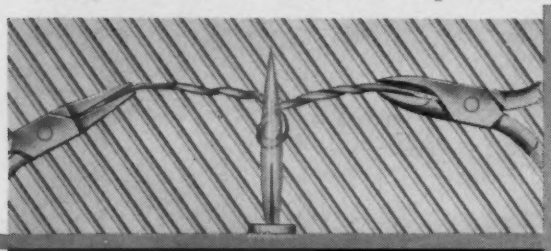

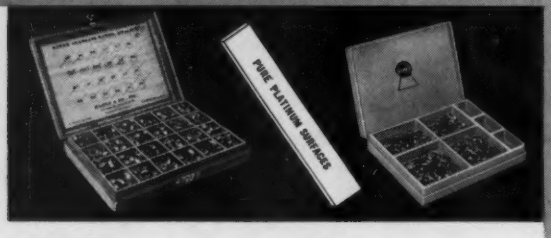

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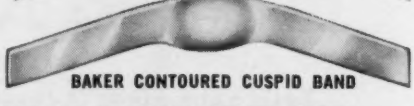

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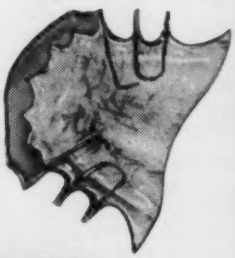

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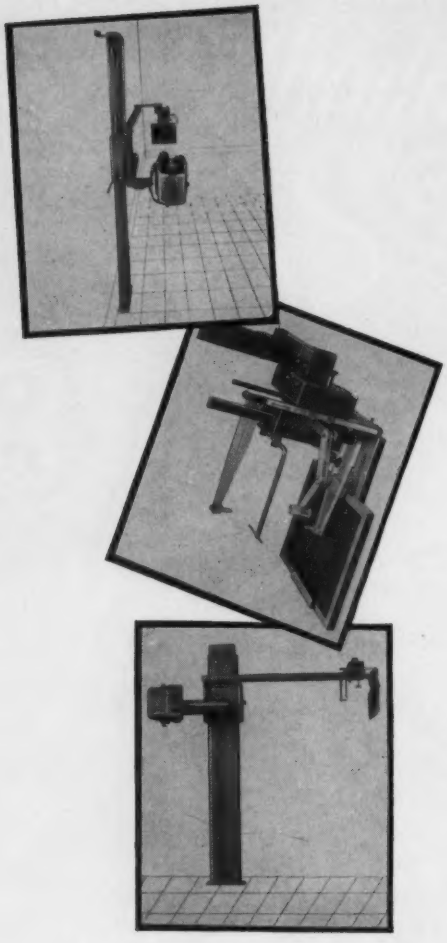

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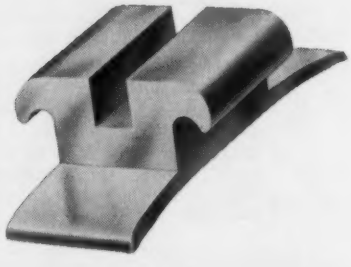

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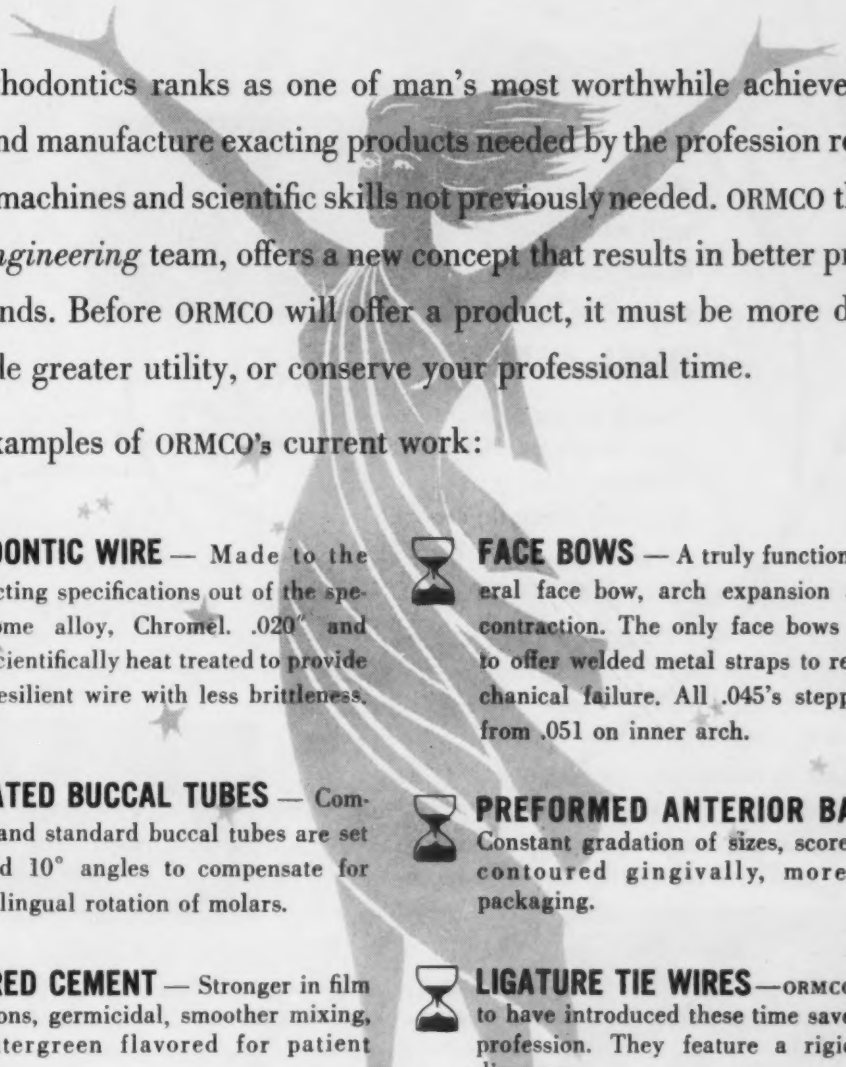

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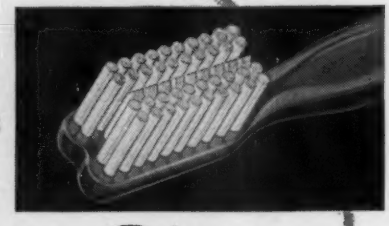

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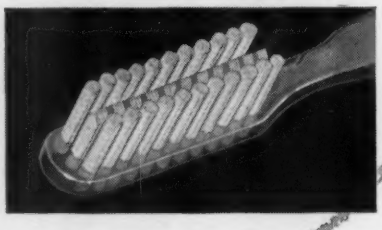

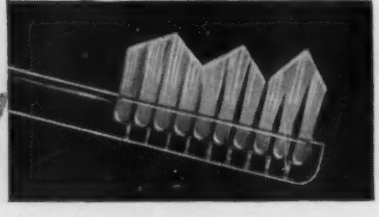

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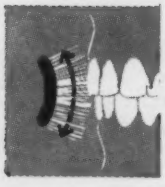

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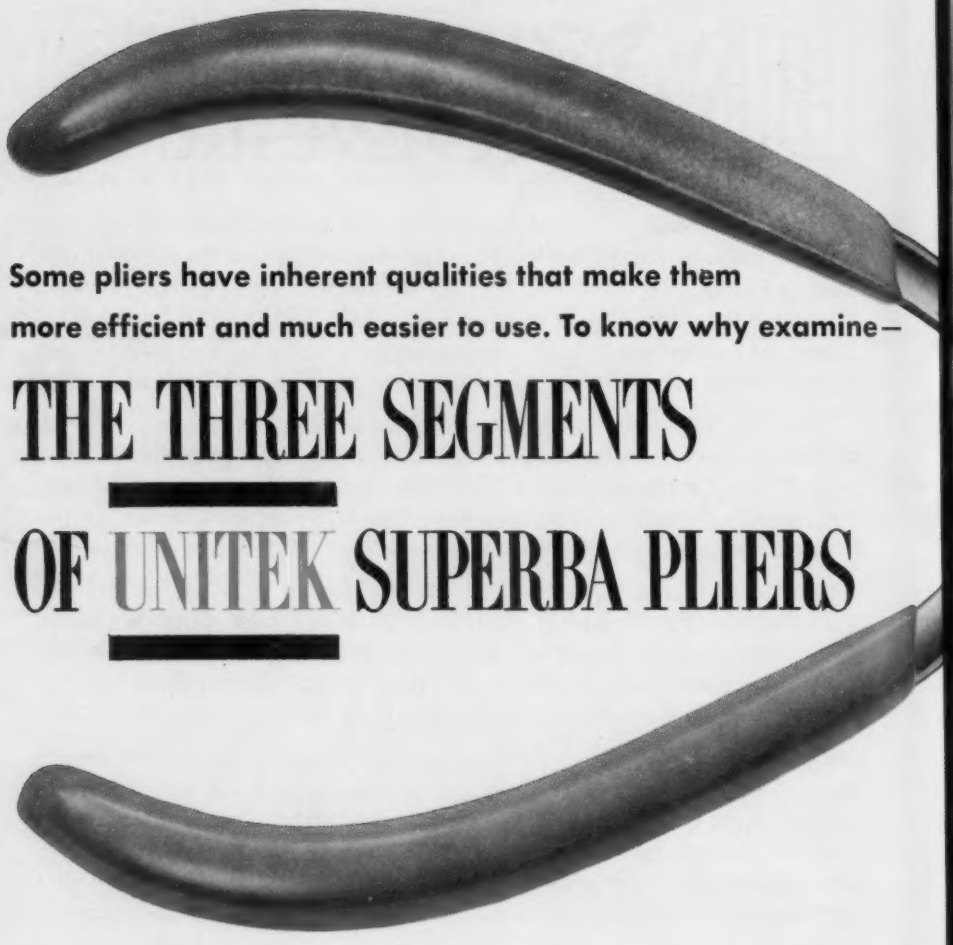

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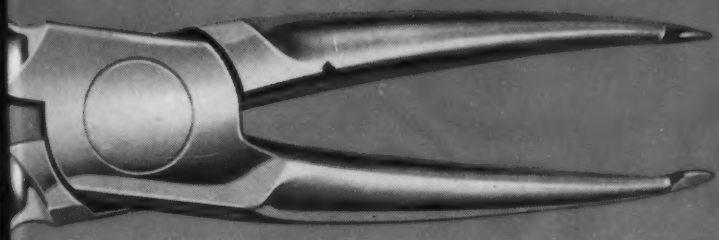

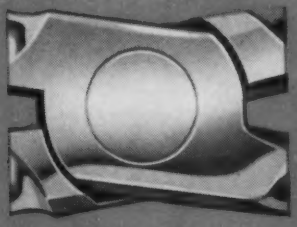

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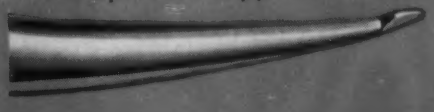

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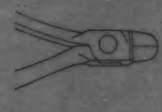

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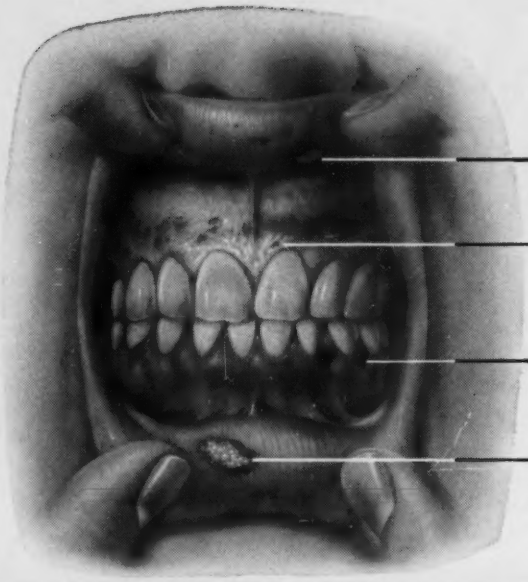

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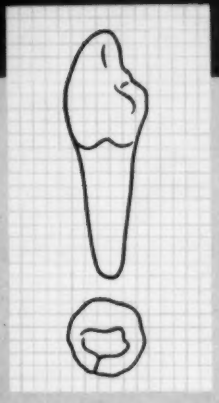

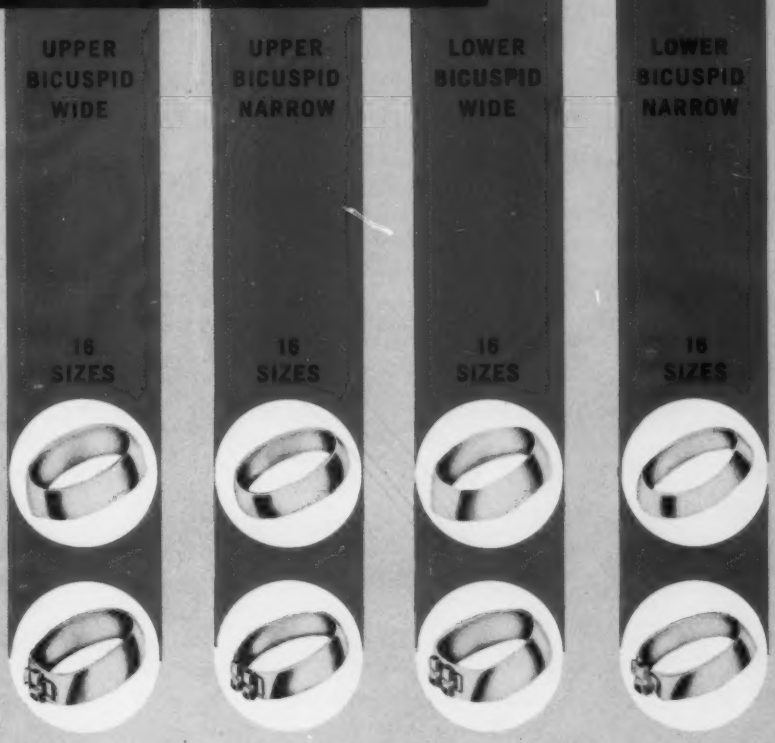

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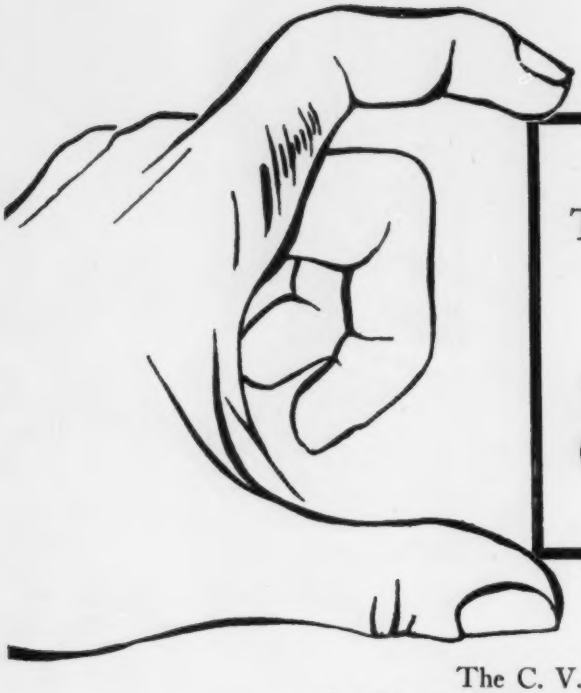

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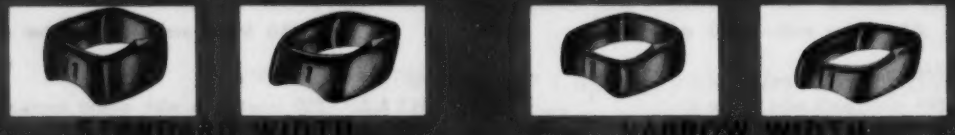

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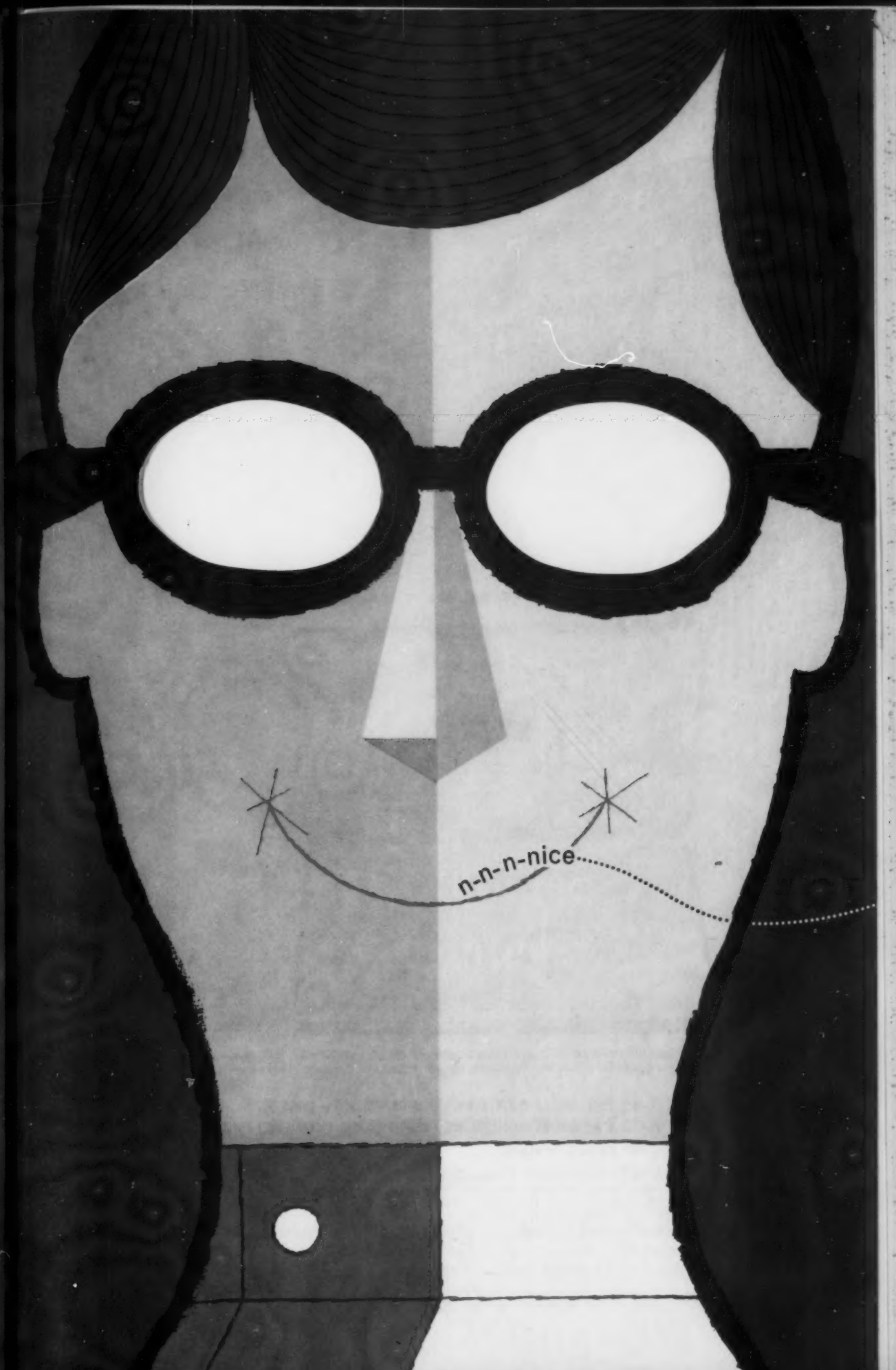
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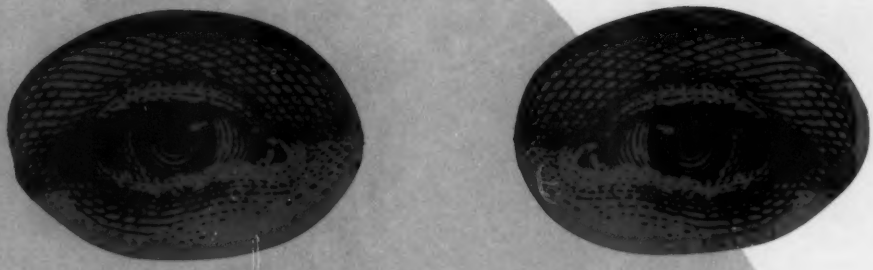

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American Journal of ORTHODONTICS

Volume 47, *Number* 10, OCTOBER, 1961

ORIGINAL ARTICLES

President's Address, Pacific Coast Society of Orthodontists

E. ALLEN BISHOP, D.D.S.

Seattle, Wash.

I HAVE thoroughly enjoyed being your president. It is too bad that everyone cannot serve in this capacity once in his career. It has been rewarding to me simply to have had the opportunity to meet so many new members and to know the older ones better.

My duties have been made easier by the excellent administration of our past president, Richard Railsback, and our secretary, Warren Kitchen. Many of their suggestions for the better functioning of our Society have been already used.

The Pacific Coast Society has had a tremendous development during the past ten years, having grown from about 240 members in 1950 to 597 in 1961. Consequently, the duties of our secretary have multiplied many times, so that in the foreseeable future he will need additional secretarial help in order to attend to the numerous details required in the efficient administration of the duties of his office.

One of the most important committees within our Society is the Membership or Qualifying Committee. In years past it was a comparatively simple procedure to become a member of our Society. Now, however, through advanced orthodontic education offered by our universities and preceptorships, younger orthodontists are much better prepared than we were years ago. As a result higher educational qualifications are demanded for membership into our orthodontic societies. In fact, within a few years, an orthodontist will have to meet these educational requirements to be recognized as a specialist within our profession. Your Membership Committee, made up of Drs. Fraser, Chipman, Muchnic, Spears, and Smith, with Dr. Hahn as chairman, has done and is still doing admirable work toward upgrading our specialty. Dr. Muchnic, as a result of additional work within our Society, has been recently replaced on this committee by Dr. John

Presented before the Pacific Coast Society of Orthodontists in Seattle, Washington, Aug. 7, 1961.

Rathbone. At this time we should give the members of the Membership Committee a generous vote of thanks for the many hours and the personal sacrifices of energy which they have expended in behalf of our profession.

In the many years in which I have been a member of the Pacific Coast Society, it has been gratifying to observe the gradual changes that have taken place within our membership. It may be expected, and it is natural, that within a dynamic specialty such as ours when a new or different philosophy is introduced positive positions are taken by members favoring one or the other schools of thought. This attitude has resulted in the possible weakening of our specialty. Within the past few years it is noticeable that the personal prejudices among the members of the several groups have been materially eliminated. This is as it should be, and it is good.

As mature members of our profession, we should strive continually to become better orthodontists and to achieve better results for our little patients. It is natural that attaining this goal, we are bound to use different philosophies of treatment to satisfy our individual personalities. With this in mind, your Program Committee has planned the program, feeling that there should be something of material value for each one of us. I am sure that will be the case at this meeting and that you will return to your offices satisfied that the time has been well spent and very much worth while. Dr. George McCulloch and his committee have worked to bring this about.

I would like to take this opportunity to discuss a change in the time of the meetings held by the Pacific Coast Society and also by our components. Our Society is the second largest of all the constituents within the American Association of Orthodontists. At the same time, it is larger in area than all others. When we were first formed, there were relatively few orthodontists in the areas north of California, and transportation between sections was much slower than it is today. Therefore, it was thought expedient to have meetings of the Pacific Coast Society held every two years and meetings of the Southern, Central, and Northern sections held from two to four times annually. With these and the annual A. A. O. meeting, it now seems that there are an "awful lot of meetings."

This number very possibly minimizes the value of each meeting, due to the Program Committee's inability to obtain enough worth-while material.

Today the speed of transportation is such that an annual meeting, supplemented by a single sectional meeting, could be held. In that way the number of our meetings could be reduced and our program material could be condensed into two meetings a year instead of the many that we now hold.

This change would also remove the attendance competition from the annual A. A. O. meeting. It would allow many of our younger men who, for obvious reasons, may feel that it is impossible to be away from their offices so much to attend the national meeting. Many of us on the Coast may have become slightly provincial simply for the reason that we have not attended many of the A. A. O. meetings. I am suggesting this change for your consideration.

Your entertainment committee with Dr. Dohner as chairman and Mrs. DeButts in charge of entertainment for the ladies, has arranged for your enjoyment and for that of your family, and our hope is that you will have as much pleasure as we have had in planning it for you.

Dr. Robert DeButts, as general chairman, and all the members of his committees have worked diligently to make this meeting outstanding. May I extend my thanks to those who have served in any way. It is our wish that you will all return when it is our privilege to again have the meeting in the Northwest.

703 Cobb Bldg.

One approach to deviate swallowing

RICHARD H. BARRETT, B.A., M.Ed.
Tucson, Ariz.

IT IS A cardinal sin in public speaking to begin with a negative statement, so I will state *positively* that in this presentation I am *not* going to discuss the specific techniques of therapy for reverse swallowing. I would like to discuss some of the circumstances underlying such treatment, but I have no wish to mislead those of you who have other expectations. I am sure you appreciate the fact that we could hardly make a beginning in a single hour. Nevertheless, I invite you to join me in a glaring *second* look at this shared problem. Many speech therapists, and some orthodontists, are still admiring the trees and have not yet entered the forest. Others of us have ventured ahead, bearded this monster, and, led in many cases by Dr. Walter Straub, have begun a slow investigation of the terrain.

Truly, we can see how necessity is the mother of invention. Most of our progress to date in the treatment of deviate deglutition has been the result of an urgent and immediate need for answers; some of us have felt that we *had* to solve this problem, and quickly. In almost impromptu fashion, we have devised procedures and techniques with which we could rush into action. There has also been an air of frenzy, almost desperation, in the attitude of some orthodontists scurrying about in search of someone to help them combat excessive tongue pressures.

We may also note that "contentment is the smother of invention." The speech therapist who successfully treats a few cases tends to standardize his program. The orthodontist who secures the services of such a therapist tends to dismiss the problem. A few orthodontists in this country still feel that tongue retraining is unnecessary, and so they ignore the whole area. Others are convinced, as are some British authorities, that treatment of these cases is completely futile, and they avoid all therapy—orthodontic or swallowing—whenever possible.

Presented at the biennial meeting of the Pacific Coast Society of Orthodontists in in Seattle, Washington, Aug. 6 to 10, 1961.

It will be the purpose of this article to look at portions of this philosophical jungle and to suggest a course of action which might open a trail down which this small aspect of orthodontics could travel in comfort and safety. It is in no danger of being trampled to death, but it is threatened by mold and stagnation and the danger of being lost in a strange and unexplored area. Every point of view around the entire periphery appears to have adherents; the trouble is that the adherents tend to remain securely in the surrounding clearings, flipping opinions, like so many pebbles, into the underbrush but not plunging into the wilderness to carve out paths of knowledge by which their fellows may proceed.

It is hoped that the difference between *stirring discontent,* with the unpleasant overtones implicit in that term, and *avoiding complacency,* a positive concept which guides the present statements, will be obvious. There is no question of the propriety of stating an opinion; I intend to offer a number of them. Situations inevitably arise, however, in which opinions must be supported by proved facts, by controlled research conducted so carefully and reported so accurately that other men in other places can prove their validity by duplicating the procedures and achieving the same results. In this respect, in the thirty or forty years since the term "tongue-thrust" first appeared in print, orthodontics has little to show on the subject. The literature may be reviewed with small profit. Perhaps the practicing orthodontist cannot be criticized for this; by and large, it is the responsibility of the training center, of the graduate school, to provide research. Yet it seems to a stranger in the field that your specialty has built itself, from infancy to its present magnificent stature, mostly through the efforts of individual orthodontists who have continually striven to improve their techniques and therefore their results.

I will present here a series of questions, together with what I believe to be the answers. My belief is based in some cases on rather informal research and in other cases simply on clinical observation of more than 1,200 cases of deviate swallowing. My impressions may prove wrong in every case. Each of these questions is open to controlled research, and in most instances the pilot studies—the guidelines—could most readily be the product of sincerely interested orthodontists observing, inquiring, and keeping records.

What, then, are the pertinent questions to which we should address ourselves? Our point of departure should be an area of common agreement, and it appears that few orthodontists dispute the concept that some children swallow in such a manner that their tongues are against or between their teeth, thus exerting an abnormal pressure in a vertical or labial direction. Why do they do this? The depth of our ignorance is manifest in the fact that no one can conclusively answer this basic question beyond dispute. Certainly I cannot. Straub[1] and Picard[2] have written on this subject, and all my experience leads me to accept their thesis, as far as it goes. This point of view, that improper bottle feeding is the primary etiological factor is further supported by the periodic reports issued by certain orthodontic supply companies,[3] indicating that this problem might be prevented in the main by the use of an improved nipple. I would like to expand this concept to include other conditions which cause liquids

to flow into the infant's mouth too rapidly and to incorporate cases in which additional causative factors are apparently operating. This entire article could easily be devoted to a discussion of this one question, in which case, if the veterinary allusion may be excused, it would resemble Aristotle speculating in classic philosophical manner on the number of teeth in a horse's mouth. Eventually, someone counted the horse's teeth. Eventually, someone must enumerate and define, statistically, the causative elements of deviate swallowing.

It may be permissible to consider briefly one or two aspects of etiology. For example, why do not all children fed with conventional nipples have deviate patterns of swallowing? It may help in our understanding if we view this question in the manner of the speech therapists, who are accustomed to thinking of the causation of stuttering on three levels: predisposing factors, precipitating factors, and perpetuating factors. A similar hierarchy may govern in cases of aberrant deglutition; it would not be illogical to postulate an emotional trait which would trigger this response in one infant but leave another functioning in a normal manner. In identical circumstances, not every child develops a lisp, but some do, and lisping is closely allied to deviate swallowing in many cases.

What of the proposition that we are looking at reverse swallowing in reverse, and that the malocclusion causes the swallowing anomaly, and not *vice versa*? This places the matter on a much more physiologic basis, and proponents of the idea must first demonstrate that all patients with Class II, Division 1 malocclusions, for example, display deviate swallowing patterns. They must likewise explain the rather large number of cases in which malocclusions of this type are accompanied by perfectly normal swallowing habits. An improper manner of deglutition is either a learned activity or it is imposed by the surrounding structure. If structural factors govern, similar malocclusions must produce the same fault in swallowing in all cases. Furthermore, if we are to dismiss the need for habit retraining, which is an integral and constituent part of this concept, the advocates must show that tongue muscles automatically reposition themselves and spontaneously function in a contradictory series of movements and operations simply by closing the dental alignment. The embarrassing number of prolonged treatment periods and of orthodontic relapses stand in irritating evidence against such a sanguine and idyllic view.

Are swallowing patterns inherited? Perhaps. It would be foolhardy to deny that this is the explanation in some cases. It seems equally rash to accept this as a universal or even a prevalent condition. Certainly, it should not lure us to the defeatist position typified by Ballard,[4] who sees no hope of treating these cases. Ballard's reasoning is that some children inherit a musculature which dictates from birth the classic development of retracted mandible, upper incisors in labioversion, everted lips, with a protrusive tongue resting interdentally in order to form a labioglossal anterior seal in swallowing. These manifestations occur because such postures require the least possible energization of the muscles involved, and any conceivable realignment or repositioning of the congenital posture is doomed to relapse since maintaining such changes requires conscious exertion and thus cannot be made habitual.

Several considerations inject themselves in this regard. There are, of course, children who reflect the lingual abnormalities of a parent. The number of such cases does not appear to exceed 10 per cent of all swallowing deviations, although this should be determined experimentally. Moreover, even in these cases, it must be established that the parent was not subjected to the same influences as the child and thus caused to learn the same undesirable habit, in which no guilty gene could be cited as the culprit. As for the futility of training a habit which requires, for a time, a certain degree of effort, we may as well say that children must remain supine, for learning to stand upright requires great force and holding the human head erect would seem well-nigh impossible if we scrutinized only the inadequate muscles and structures by which we routinely accomplish this feat. My impulse is simply to point to over 1,000 successfully treated cases and wonder how we managed, if it is impossible.

This statement gives immediate rise, however, to a series of other challenges. Can new swallowing habits be made permanent? How do we know when they are corrected? How do we know that they were wrong to start with? What is normal swallowing, and how far may this deviate before it is considered abnormal?

Here again, the need for research is immediate and urgent. Until the normal process is more clearly delimited, we can make only gross judgments, since present accounts of the normal swallowing act are only gross observations. We need detailed muscle-by-muscle descriptions of function, of movement, not static position, together with some sort of quantification of badness and goodness. We need a standardized classification of the various types which occur, for there are many ways in which a swallow may go astray. Failing this, we can be only approximately sure that therapy has instilled a correct pattern, and we must continue to delay therapy in some cases, fearing that it is not justified, until the lack of orthodontic progress demonstrates the necessity. It appears that the cinefluoroscope offers the greatest hope at the moment, and a number of studies employing this device are now under way. Unfortunately, in some cases the experimenters have not been sufficiently acquainted with the problem to recognize some of the minor differences which, perhaps, are significant.

Eventually, we must agree upon some criteria for judging the results of the speech therapist's program. Should we be content with simply relieving tongue pressures upon the teeth during the act of deglutition? Certainly, this single outcome is not to be scoffed at, and once it comprised the full scope of our efforts. Closer acquaintance reveals some other aspects of this problem, however, which demand our sober thought. Associated sucking habits sabotage some cases. Should the orthodontist attack these thumbs and fingers with a crib or expect the speech therapist to make a place for such correction? The coercion implicit in any mechanical device may do irreparable harm emotionally, or it may call forth a substitute habit which is more objectionable to the parents than the original habit. Orthodontists and therapists must often confer on individual patients, for there is no universal solution. Cooperation must be especially close in some cases of lip-sucking. It appears that the therapist may be safely entrusted with routine thumb-sucking situations.

The matter of facial musculature must be resolved. We do, of course, teach children to swallow without using such muscles. In many cases, however, a perfectly retrained habit of deglutition is accompanied by lips that are so weak and so malpositioned that even normal tongue pressure cannot be offset. It seems to me that this factor must be incorporated in the speech therapist's routine if his therapy is to be considered complete and adequate.

There remains the influence of speech itself upon tongue movements. Any type of lisp, or any speech defect involving sounds which require a lifting of anterior tongue segments, can have a direct and adverse effect upon the permanency of newly acquired habits of deglutition. Therefore, it should be expected that in all cases the therapist will correct such speech disorders prior to the termination of therapy. At the same time, the orthodontist should realize that certain speech sounds demand contact between tongue and teeth and can be produced in no other way. Any goal which looks to complete absence of linguodental contact is unrealistic. The magnitude of the pressures involved in speech, chewing, and just living is relatively minor, and an adequate lip structure should easily compensate for this. The average tongue occupies a major portion of the oral cavity; short of glossectomy, it cannot be made to disappear.

We must not avoid the question of the feasibility of attaining permanency of these new habits. In this instance, there is a somewhat more definite answer. I have recently completed a limited survey of cases treated in my office during 1959 and before, or two to three years ago. I readily admit that we did things the hard way at that time. Even so, more than 95 per cent of our patients successfully mastered the mechanics of an acceptable swallow and appeared to be using it when checked one to three months following therapy. It now appears to be no great accomplishment to convey the mere mechanics of normal swallowing; making it habitual is another matter. The purpose of this survey was to determine to what degree normal swallowing had been maintained during the intervening time. Careful individual examination showed that 76 per cent of the patients display satisfactory performance today; the majority were seen prior to orthodontic treatment, then treated, and retained, and there was only one orthodontic relapse in the group. Another 20 per cent showed some mild deterioration of the deglutition habit, were retreated on the spot, and have a very good prognosis. The remaining 4 per cent were considered to be total relapse cases. Of course, these are merely subjective evaluations at this point. However, obvious changes have been made in the original movements. I think there is really no question that the job *can* be done.

The basic question appears to be *how* the job should be done. By what methods and procedures can the problem be most efficiently handled from an orthodontic point of view? The answer to this question will occupy the balance of this article.

First, let me outline briefly the program in my own community. Two other speech correctionists are associated with me, so that we can accommodate our regular case load of speech defects proper in addition to the patients referred

by ten orthodontists and a somewhat larger number of pedodontists and other dentists. Although it is in a medical-dental building, my office is not associated with that of any physician or dentist.

Routinely, the orthodontists examine for deviations in deglutition during the initial interview. If a problem exists, the orthodontist usually makes an immediate referral, advising the patient that orthodontic treatment can begin only when the swallowing habit has been corrected. In some cases, preliminary orthodontic care will be indicated, in which event the patient is informed that habit therapy will be required before the completion of treatment. A short explanation of the condition is presented by the dentist, who stresses the importance of habit correction to the orthodontic program.

A detailed exploration of the problem and an over-all view of the necessary therapy is included in the first interview at my office. Following the second or third visit, reports are sent to the orthodontist. These reports include details of progress and cooperation, together with any observations which might be of benefit to the orthodontist; unusual situations are discussed with the orthodontist by telephone or in conference. At the conclusion of therapy the patient is dismissed for a period of three to four weeks, after which he returns for a recheck. If he appears insecure in the new habit, additional measures are employed. When adequate mastery has been achieved, a final report is submitted to the orthodontist and he proceeds with treatment. The patient continues to return at three-month intervals for further rechecks until both the dentist and the speech therapist are convinced that the new habit is established.

What are the considerations which have led us to this particular type of arrangement rather than one of a variety of alternatives? First of all, who, specifically, should do this job? Is it wise to involve a speech therapist in this work? Should the orthodontist provide the therapy, either personally or through his dental assistant or laboratory technician? What other solutions are available?

The position has been pressed that if this is an orthodontic problem the orthodontist should personally administer the corrective measures but that if it is not an orthodontic problem the orthodontist is not justified in assuming any role in the entire matter. This attitude appears completely without foundation. In medicine, dentistry, speech pathology, psychotherapy, and all allied fields, the practice of referral to a qualified specialist is basic. In fact, stigma is attached only when there is failure to make the indicated referral. If you are in exclusive practice, and caries are evident when you prepare to apply the bands, would you fill those teeth or make an extraction? Your responsibility extends to every facet which could make your treatment quicker, safer, and better. Deviate swallowing presents a definite barrier to effective treatment in many of your cases. Who but you is able to advise and counsel these patients? It is sheer dereliction to ignore this condition in the light of present knowledge. The day has not yet come when adequate provision can be made for all these patients, any more than all communities can provide ideal surgical care for cleft palate cases, but certainly efforts in this direction must be sustained.

Simple practicality eliminates the orthodontist as "swallowing therapist" in most instances. You cannot afford the protracted time required for therapy. Many of you would have neither the interest nor the inclination to worry through these procedures regularly. While you could readily acquire the special knowledge and skills which are needed for this work, and you certainly should acquaint yourselves with the field if you are to discuss it intelligently, the techniques involved are distinct from any aspect of training in orthodontics and some effort is required in perfecting them for daily use. Thus, it seems illogical to nominate the orthodontist for this position.

How about the girls employed in your office? Without doubt there are some exceptional cases in which such a person displays an unusual aptitude or happens to have an appropriate background of training or experience which would permit her to obtain satisfactorily consistent results if given specific training in deglutition habits. Such persons are rare. Moreover, if there is one aspect of deviate swallowing of which we are becoming thoroughly and uncomfortably aware, it is that this is a complex and resistive adversary, not a simple matter of telling children to keep their tongues away from their teeth. True, there are some cases in which mere explanation and admonition seem sufficient to effect a complete "cure." These cases are also rare. More commonly every resource at our command seems little enough for success, and the other extreme produces children impervious to the most expert and intensive care. Probably the most vicious aspect of the old "hay rake" approach was that it *did* work in a few cases; likewise, the personnel of the dental office may occasionally produce a satisfactory outcome. Most orthodontists require a higher degree of consistency in order to feel that they have fully discharged their obligations.

In college communities what should we expect from the university speech department? We must realize that these departments have three goals: to train students, to provide research, and to render such service as they are able to the community. Such clinics exist primarily for the purpose of training students. Patients accepted for treatment must be selected with an eye to providing needed experience for the student clinicians in a wide variety of speech disorders; each student, provided that he had received instruction in swallowing therapy, would have time to see only one or two patients referred by orthodontists. This is a learning experience for the student, and, while a few patients may receive some help, it is a limited solution. Graduate students engaged in research may, in some cases, correct a number of deviate swallows if this is directly related to their research problem. This is a temporary solution. The desire to render a service to the community must remain a supernumerary aim at most colleges, possible of achievement only within the confines of the basic organizational purposes.

Please note that I am speaking in behalf of the universities, not in opposition to them. Most have not yet had an opportunity to organize formal courses of instruction in swallowing therapy; they need a little time. Students cannot safely rehearse their therapy by any satisfactory method other than practice in the clinic; they should see some routine swallowing cases. The research that

we seek can never be completed without graduate student studies; these students should have subjects with which to experiment. It is not realistic or fair to expect a university speech department to accept without resentment a flood of referrals beyond the scope of the department's intended function. However much they may wish to oblige, they can only share your frustration at the moment.

What recommends the speech therapist for this role? Consider first that there are no such things as "speech organs," as such; there are only those organs whose basic functions are sucking, chewing, and swallowing. Speech pathology, uniquely among the professions, is focused upon these muscles and structures, their anatomy and function, their adaptations and coordination, their disorders and limitations, their potentialities for change, and the techniques by which to secure such change. The speech therapist's supportive training usually includes psychology, specifically child psychology, and its applications in modifying the behavior patterns of children. Even so, some additional education is customarily required concerning the specific malfunctions of deglutition and the orthodontic implication involved, but the eventual inclusion of these data in the regular training program for speech therapists seems inevitable. There appear to be implications for articulation, as well as orthodontics, in this problem, and the therapeutic alliance between dentistry and speech correction is destined to grow closer as new knowledge is acquired. Some of the basic research is already under way in the field of speech. This entire armamentarium is placed eagerly at the disposal of any orthodontist who wishes to avail himself of the service.

Here, again, some pertinent questions arise. Can *every* speech therapist do this work? Probably not. Ours is a rather broad field and many have specialized in incompatible areas. Others are engrossed in teaching or in specific projects along other lines and lack an interest in this particular problem. Still others function best in positions in which they are supervised by a more mature therapist, and some have not yet progressed far enough in their work to produce optimum results. The vast majority are eminently qualified.

Should you then hire a speech therapist, have him trained in swallowing therapy, and retain him as a salaried employee? There are certain advantages in this arrangement. It assures proper attention to your patients. It provides a certain prestige and a measure of flexibility and control over timing, methods, evaluations, etc. Ideally, there should be the closest possible cooperation between speech therapist and orthodontist, with the records of each available to the other, with all aspects of treatment planned to coincide with the best interests of the patient, and with the opportunity to discuss unexpected developments. However, so many factors of such commanding importance militate against the hired therapist that this plan cannot be recommended. Provision can be made for almost all of the advantages when the therapist is in private practice. The orthodontist is still the "doctor," with responsibility for the decision as to who shall be referred and when. The added self-respect of the therapist-on-his-own results in an improved attitude which portends greater effectiveness in his work. Whereas working for numerous "bosses" would prove intolerable, the increased

earning power that he enjoys when referrals can be accepted from several orthodontists is a major incentive. If the therapist is independent, any failure of rapport in one office need not be imposed on the other. Most speech therapists would soon become at least mildly psychotic if deprived of the opportunity to intersperse a few legitimate speech defects among the "tongue-thrusters."

Most important, perhaps, is the altered relationship between therapist and patient. I like working with children, and most of them learn to enjoy the exercises that are required. We make it as much fun as possible, and a few have actually wept when therapy was completed. Yet I know of no nondirective approach to swallowing therapy at this time. The authority of the therapist must not be impaired in any way if the average case is to be corrected. We do not work with uncooperative children in my office, although some of the children start out that way. I think that we might validly be accused of unethical behavior if we continued to accept fees from patients who obviously are not going to complete the modification of their habits. Therefore, as soon as such a situation becomes apparent, we tactfully suggest that the patient drop therapy until such time as he is willing to follow the program, and we advise the orthodontist of the circumstances. The mere threat of dismissal is sometimes sufficient to bring about a change in attitude, but more commonly the patient returns to the orthodontist and seeks braces without habit therapy. Our orthodontists have remained steadfast in refusing to begin therapy in this way, and almost without exception the patient has returned and learned to swallow properly. The total number of such cases is small, but it would probably increase under the hired-therapist plan. Even with the full consent of the orthodontist to manage such patients in any way that the therapist's discretion indicated, the restriction of being in the orthodontist's office, treating *his* patients on *his* terms, dampens the positive approach which can be of benefit to all—orthodontist, therapist, and patient. Most speech therapists have a moderate income. You may help them get started, even pay for their special training, and find that you are quickly repaid through the product of their services. But there is no compensation for the added problems you acquire by putting them on your staff.

Another solution is to leave the entire problem with the public school speech correctionists. This simply will not do. One could quickly fill pages with reasons why this is impractical, inefficient, morally wrong, and redounding to your own detriment. I know that this has been tried in many places, but I am not aware of a single case in which it has proved satisfactory. This is in no way a reflection upon the public school speech correctionists. They get better results than could be expected doing the job for which they are employed, but correcting reverse swallowing patterns is in no way a part of that job. I will not pursue this subject further, but I would be glad to meet the contrary minded in a friendly extemporaneous debate.

It now appears that all of the considerations discussed here will cease to be problems when orthodontics and speech therapy join forces in a unified attack. Each has a contribution to make; each has need of the special skills of the other. The speech therapist is faced with more immediate preparation, for he must

learn something of your terminology and your procedures, of cephalometrics, and of the literature in your field. Yet many of the minor problems which vex you are handled routinely in speech practice. Together you can plan and execute, experiment, revise, and alter, evaluating by the exacting methods which only you can provide.

A body of data concerned with the purely speech aspects of deviate swallowing is now being accumulated in speech clinics across the country. This will probably be characterized by a welter of minutiae which demand some patience of the practical minded. Nevertheless, broad answers will emerge, and this information should be available to the orthodontist.

Some of the academicians have been highly critical of both our professions, stating that we should not undertake to treat a condition about which we know so little. First, they say, complete the basic research and then write papers for conventions and begin to apply therapy. This is a very safe position. But when do you suppose we would get around to doing something for the children who daily inhabit your operatories? Within their lifetime? Or ours? Their parents are paying fees for which they deserve the best treatment obtainable. Can such treatment safely ignore this entire condition at the present time? Can you, in good conscience, step aside and await the day when speech therapists have solved this riddle unassisted? Their research is now under way, but it can do justice to the orthodontic aspects of the problem only with your assistance. Could not this entire matter be speedily resolved and the appropriate applications made by a concerted effort within the next very few years? I think the time has come to lay aside prejudice and idle speculation, to make your second look at deviate swallowing a searching one, to *find out* about this condition objectively, and then be able to act realistically in the light of this knowledge.

The roster of questions considered here is far from exhaustive; the first draft of this article contained almost twice as many. Nor were these selected as being safe for discussion or calculated to endear me to this audience. While it seems to me that nothing is unanimous in orthodontics, the subject of deglutition is particularly controversial at this time. I knew when I accepted this assignment that there is no "safe" approach to this topic. In no event could I come before you with sweet-coated platitudes—you know what sugar does to your teeth. I apologize also if I have set your teeth on edge. I am profoundly honored by the opportunity to address this meeting. It is futile to attempt to express my appreciation, and for this very reason I could do no less than set forth my sincere convictions. If the opinions which I have stated have offended some of you, I am deeply sorry. I look forward to the day when we know all the answers, and I await with some impatience the time when we can prove even a few. Those halcyon days will only arrive if we face this problem clearly, open-mindedly, inquiringly, and in good faith. Emerson saw that much of our trouble arises from fear, the fear that always springs from ignorance. He counseled that the scholar should allow "no diversion of his thoughts from vexed questions, hiding his head like an ostrich in the flowering bushes, peeping into microscopes, and turning rhymes, as a boy whistles to keep his courage up. So

is the danger a danger still; so is the fear worse. Manlike let him turn and face it. Let him look into its eye and search its nature, inspect its origin—see the whelping of this lion—which lies no great way back; he will then find in himself a perfect comprehension of its nature and extent; he will have made his hands meet on the other side, and can henceforth defy it and pass on superior.''

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612 N. 4th Ave.

Nonextraction treatment in growing patients, with emphasis on distal movement

LOUIS S. MILLER, D.D.S.
Salt Lake City, Utah

INTRODUCTION

SOME of the most basic and provocative articles ever to appear in the orthodontic literature were the articles written by Hays N. Nance on the limitations of orthodontic treatment.

Nance's study consisted of an analysis and comparison of measurements taken from models at various stages of development. These records were taken from both treated and untreated cases and from the offices of many participating orthodontists. The information was collected over a period of twenty-six years. Measurements were taken from the models of the same cases before treatment and again after many years had elapsed.

Some of the fundamental points brought out in these papers[1] are as follows:

1. If a stable, permanent result is to be attained in orthodontic treatment, mandibular teeth must be positioned properly in relation to basal bone.
2. Arch length may be permanently increased to only a limited extent. The labial movement of mandibular anterior teeth is indicated only when these teeth have been tipped to an abnormal lingual-axial inclination by a deep overbite or by some other mechanical factor. A limited amount of permanent increase may be obtained through buccal expansion in some cases, but not in all. Distal movement of mandibular molars is called for only in cases in which bona fide mesial drift has been caused by the premature loss of second deciduous molars or

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by the loss or congenital absence of second premolars. These limitations, upon permanently increasing the arch length, oblige the orthodontist to remove tooth structure in the majority of cases in which there are rotations, crowding, or anterior teeth that are tipped forward of the bony base.

3. Excessive lingual tipping of mandibular incisors is to be deplored as much as excessive labial tipping of these teeth.
4. The length of the dental arch from the mesial aspect of one mandibular first molar to the corresponding tooth on the opposite side is always shortened in the transition from the mixed to the permanent dentition. Furthermore, this arch length cannot be permanently increased through orthodontic treatment in the mixed dentition; on the other hand, it will decrease. (Nance lists some exceptions to the rule, as will be pointed out later.)
5. The distance from the mesiolingual aspect of the mandibular first molar to the mandibular midline always shortens in the transition from the mixed to the permanent dentition. This distance cannot be permanently increased through orthodontic treatment in the mixed dentition, allowing again for the same exceptions.
6. It is a fact that the mixed dentition is characterized by an end-on relationship of the maxillary and mandibular first permanent molars instead of the usual cusp-in-groove relationship.

In studying the average mesiodistal width of teeth in Black's *Dental Anatomy* Nance[1] noted that the mandibular deciduous cuspid, the first deciduous molar, and second deciduous molar were 1.7 mm. wider than the teeth which would succeed them, that is, the permanent cuspid and the two premolars.

In his measurements, however, he found that this is not always true; sometimes there will be as much as a 4 mm. difference on each side, and sometimes there will be no difference at all. It is a good idea to observe the width of the lower second deciduous molar. The average leeway in the maxillary arch, according to Black's averages, is only 0.9 mm. This explains the end-on relationship in the mixed dentition. In making a diagnosis in the mixed dentition, it is very important for the orthodontist to note whether this end-on relationship of the first permanent molars exists, so that he will know whether or not to subtract 1.7 mm. from the available arch length.[1]

I believe that a great many mistakes in diagnosis and treatment philosophy have resulted from the failure of orthodontists to read these articles carefully enough. Incomplete understanding causes them to think that they are limited too much in orthodontic treatment. If we were to take the principles laid down as being law, without exceptions, we would find that a great majority of our cases fall into the category of extraction cases. If, however, we read the small print which outlines the exceptions to the rules, we find that Nance cites the Class II, Division 2 cases and the Class I deep overbite cases which resemble them as being exceptions to the rule.[2] Cases which present abnormal lingually tipped incisors are also cited as exceptions. These exceptions certainly make up a large group of the cases seen in an orthodontic practice.

For all practical purposes, a critical analysis of treated orthodontic cases will uphold the basic facts brought to light in Nance's articles.

It behooves us to be aware of our limitations in orthodontic treatment. In this article, however, I shall attempt to explore areas of release from the limitations imposed upon us. My efforts are being reported for the benefit of those who desire to improve the profile by means of nonextraction orthodontic treatment and to achieve a healthier, more stable denture as an end result.

I intend to demonstrate the following facts:

1. Arch length can be gained by distal tipping of teeth.
2. Lower buccal segments will stay tipped back of their original position if certain conditions prevail.
3. Lower buccal teeth can be moved bodily in a distal direction.
4. Buccal expansion is possible in some cases.
5. Buccal expansion at the distal end of the arch does little to increase arch length.
6. Mesial drift of first permanent molars in transition from the mixed dentition to the permanent dentition is not inevitable.

I shall also demonstrate the possible fate of lower buccal teeth left in a tipped-back position and the consequences of exceeding our limitations.

USE OF CLASS III MECHANICS

In many of our so-called arch length discrepancy cases we will discover, on careful examination of the models and headfilms, that an anterior tipping of the mandibular molars exists.

We can upright the lower buccal segments and correct any slight crowding or slightly procumbent position of lower anterior teeth with space made available by the use of Class III mechanics. We would then follow with distal driving[3] of upper buccal segments with cervical gear of the Kloehn[4] type and/or Class II elastics until a normal cusp-in-groove relationship of the first permanent molars exists. Can we expect these teeth to remain stable?

Fig. 1 shows an example of this type of case. Patient D. R., an 11-year-old girl, had a Class II, Division 1 malocclusion and a 4 mm. discrepancy in both arches.

Originally, I had planned to extract the lower first and upper second premolars and waste anchorage. However, because there was a large chin button and a thick soft tissue pad[5] over pogonion and because the patient had a rather straight profile, I decided to attempt nonextraction treatment. I was apprehensive because the lower first molars did not appear to be tipped forward.

My treatment plan was to prepare anchorage in the lower arch which could be used later to reduce the protrusion and gain normal molar relationship with Class II elastics. In the upper arch an 0.0215 by 0.027 inch passive gold stabilizing arch wire was adapted to the original malocclusion, so Class III force was started immediately after banding. Thus, there was a relatively undisturbed upper arch for anchorage. Headgear of the Lewis[6] type was to be worn fourteen hours each night to stabilize the upper arch. I planned to move the lower incisor distally 2 mm. before changing to Class II elastics and using the prepared lower arch.

When a progress headfilm was taken to verify the lower incisor position I noted that not only was the lower arch tipped back, but that the upper arch had also moved distally. I decided to use a Kloehn type of cervical gear to the distal portion of the upper arch and to attach a high-pull headgear of the McCulloch type to hooks distal to the lateral incisors. These two headgears[5] worked well together and the case was treated in nineteen months without Class II elastics ever being used.

A tracing of the retention headfilm showed that about a 1.5 degree opening had been achieved and that the vertical ramus height had been increased by 5 mm. The upper incisors had moved bodily about 4 mm. distally, and the upper first molars about 6 mm. according to the tracing, which was registered on the original NA plane at the palatal plane.[7]

The lower premolars and molars were quite vertical in relation to the occlusal plane, but they seemed to have tipped back a few degrees in relation to the lower border of the mandible. The upper first molar was not tipped back but moved bodily.

It is true that by uprighting lower buccal teeth and unraveling a lower arch we sometimes crowd the distal portion of the arch, but in these growing young patients that area distal to the first and second permanent molars will later accommodate the teeth which are crowded back into it. This distal tipping of lower buccal segments with Class III mechanics and then the distal driving of the upper buccal segments with headgear of the Kloehn type and light Class II mechanics are saving the so-called borderline cases from needless extraction of premolars.

Nance[1] noted:

> John Hunter, in 1771, after extensive investigation with a method which revealed precisely the sites of new bone growth, made the following statements, which are quoted by Brash: "The jaw still increases in all points until 12 months after birth, when the bodies of all the six teeth are pretty well formed, but never after increases in length between symphysis and the sixth tooth; and from this time, too, the alveolar process, which makes the anterior part of the arches of both jaws, never becomes a section of the large circle.... After this time, the jaws lengthen only at their posterior end."

The use of Class III mechanics in the treatment of Class II cases was made popular by Tweed,[8] who used this distal force to the lower arch in conjunction with second-order bends in the lower arch wire to tip the teeth back or to put them in a position to resist anterior displacement. In other words, he reasoned that, placed in these tipped-back positions, they would act much like tent pegs in resisting displacement.

Whether or not this reasoning is scientifically correct is the subject of a great deal of controversy. There are many who feel that this anchorage preparation merely gives the teeth a "round trip."

Regardless of who is right and who is wrong, the fact remains that Tweed and his followers are able to treat the most difficult cases successfully and to obtain a great deal of distal tooth movement in the maxillary arch by means of this prepared anchorage.

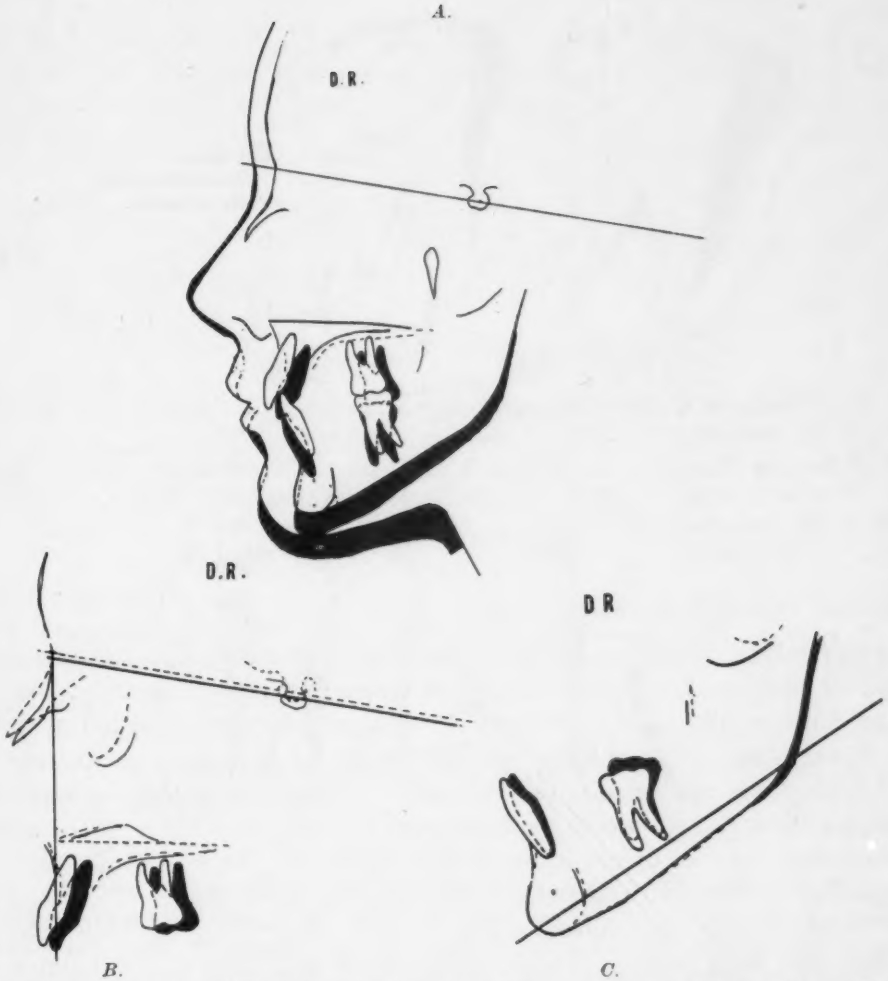

Fig. 1. Patient D. R. *A*, "Before and after" headfilm tracings superimposed on SN plane and registered on sella. Note the over-all growth picture. The patient incurred about a 1 degree opening and a 5 mm. increase in ramus height. The upper molar is vertical in relation to the lower molar.

B, Tracings superimposed on NA plane and registered on palatal plane, showing tooth movement in the maxilla. The upper incisors moved distally 4 mm., while the molar moved distally 6 mm.

C, Tracings superimposed on GoGn plane and registered on D point. The lower buccal teeth are quite vertical in relation to the occlusal plane but tipped back a few degrees in relation to the GoGn plane.

Holdaway[9] has observed that it is quite possible to have anterior displacement of the lower molar roots when second-order bends are used in anchorage preparation. This is especially true if the patient becomes lax in wearing the Class III elastics. It is also true when intermittent distal force is used, such as nightly use of headgear direct to the lower arch, or when Class III elastic force is used only at night and supported by headgear. If a tip-back force is bent into an arch wire, there must be a constant distal force to avoid anterior root displacement.

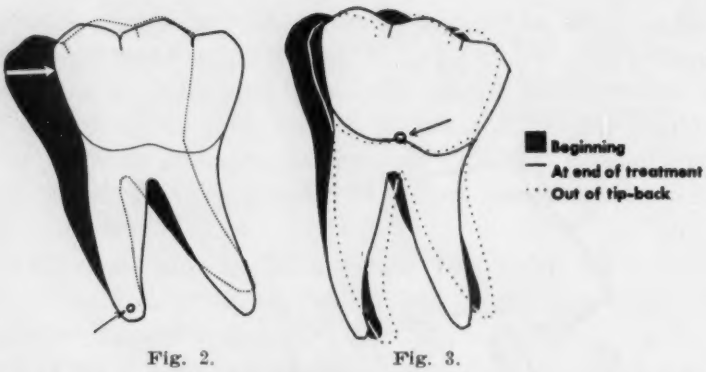

Fig. 2. Fig. 3.

Fig. 2. A silhouette of a molar, illustrating how much space can be gained by merely uprighting it with the center of rotation at the apex of the root.

Fig. 3. A drawing illustrating how a lower tipped-back molar will usually upright itself in function in a well-treated extraction case. Note that the center of rotation is at about the junction of the crown and root portion.

GAINING SPACE BY DISTAL MOVEMENT

In an analysis of models, especially in cases of crowding, it is not uncommon to discover that the lower first molars are frequently tipped mesially. One has only to draw a silhouette of a tooth in a tipped-forward position and then rotate it backward with the center of rotation at the root apex to discover that considerable space can be acquired (Fig. 2). Let us add to this the possibility that tipped-back lower buccal teeth usually upright themselves with a distal positioning of the root apices in extraction cases with the center of rotation at the junction of the crown and root portions of the teeth[5] (Fig. 3). This happened in the nonextraction case shown in Fig. 4, with roots going more distally than they had ever been before. I cite this to show only that it did happen and not that it will usually happen. Holdaway was the first to report this occurrence, and he points out that it is observed mostly in extraction cases.

Regarding orthodontic appliances, Dewel[10] has written as follows:

> In the usual crowded oral occlusion, anterior teeth will tip labially on insertion of ideal arch wires in brackets on incisors. They have no place to go but forward; it is one form of undesirable expansion. The four incisors in each arch are the weakest of all teeth and they will move labially beyond the limits of basal bone, while the larger number of sturdy posterior teeth remain undisturbed. This will occur even though the crowding is moderate. Proper correction of crowded incisors calls for distal movement of cuspids and premolars before attempting to realign anterior teeth over the apical base.

To position these crowded incisors safely over apical base in nonextraction cases, lower buccal segments should be tipped back sufficiently to open the required space mesial to the lower cuspids before the lower incisors are banded (Fig. 5).

Class III mechanics have a tendency to open the bite by extruding the upper molars. In Class II, Division 2 cases this is a very desirable way to free the mandible and allow it to move anteriorly. Many of our Class I crowded

Fig. 4.

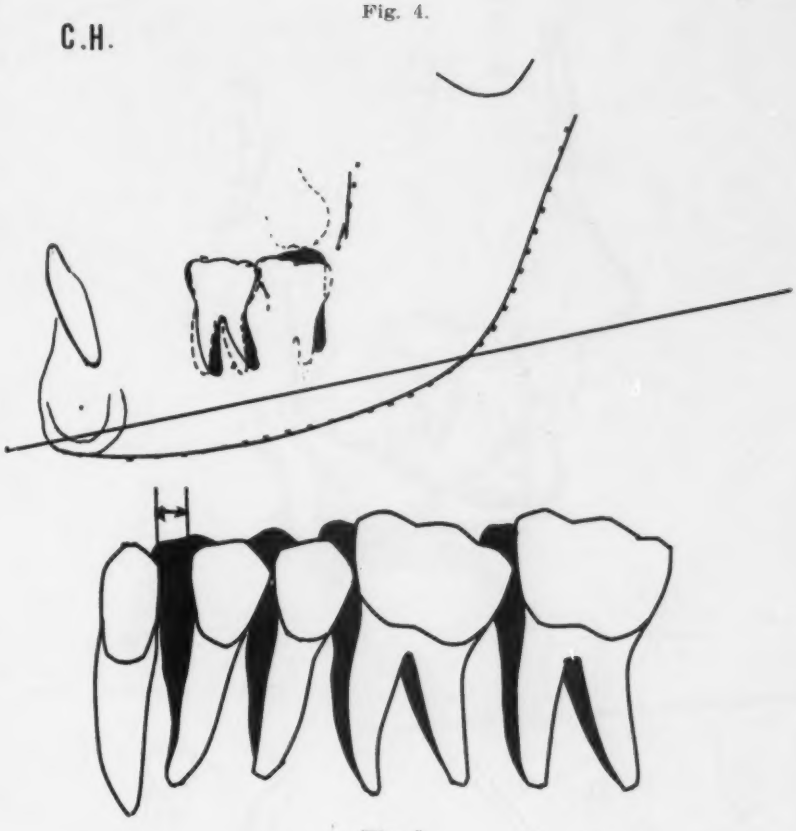

Fig. 5.

Fig. 4. Patient C. H. In this case the lower second molar uprighted itself, with the root apex going 2 mm. further distally than it had been previously. Note also that the distal cusp moved upward until it contacted the upper molar, rather than the reverse happening. The lower molar changed this much from Feb. 27, 1959, when retained, to Oct. 10, 1960. Note that there was no forward movement of the lower incisor.

Fig. 5. Drawing showing how space for crowded anterior teeth can be opened by tipping back buccal teeth.

cases are of the deep overbite type. In these cases this bite opening is also beneficial. Extrusion of the upper molars, facilitates distal movement when Class II or extraoral distal force is instituted.[5]

When it is anticipated that Class II elastics will be used in treatment, it is very desirable to tip the occlusal plane distally first so that there will not be an anterior cant of the occlusal plane when treatment is completed.[5]

Crockatt[11] showed, in a study of 100 treated cases, that anterior displacement of maxillary teeth need not be feared when Class III elastics are used.

BODILY DISTAL MOVEMENT OF LOWER BUCCAL TEETH

The bodily distal movement of lower buccal teeth is very difficult after these teeth have erupted. Nance[2] stated that this is very difficult except in cases of bona fide mesial drift caused by premature loss of deciduous teeth. In the

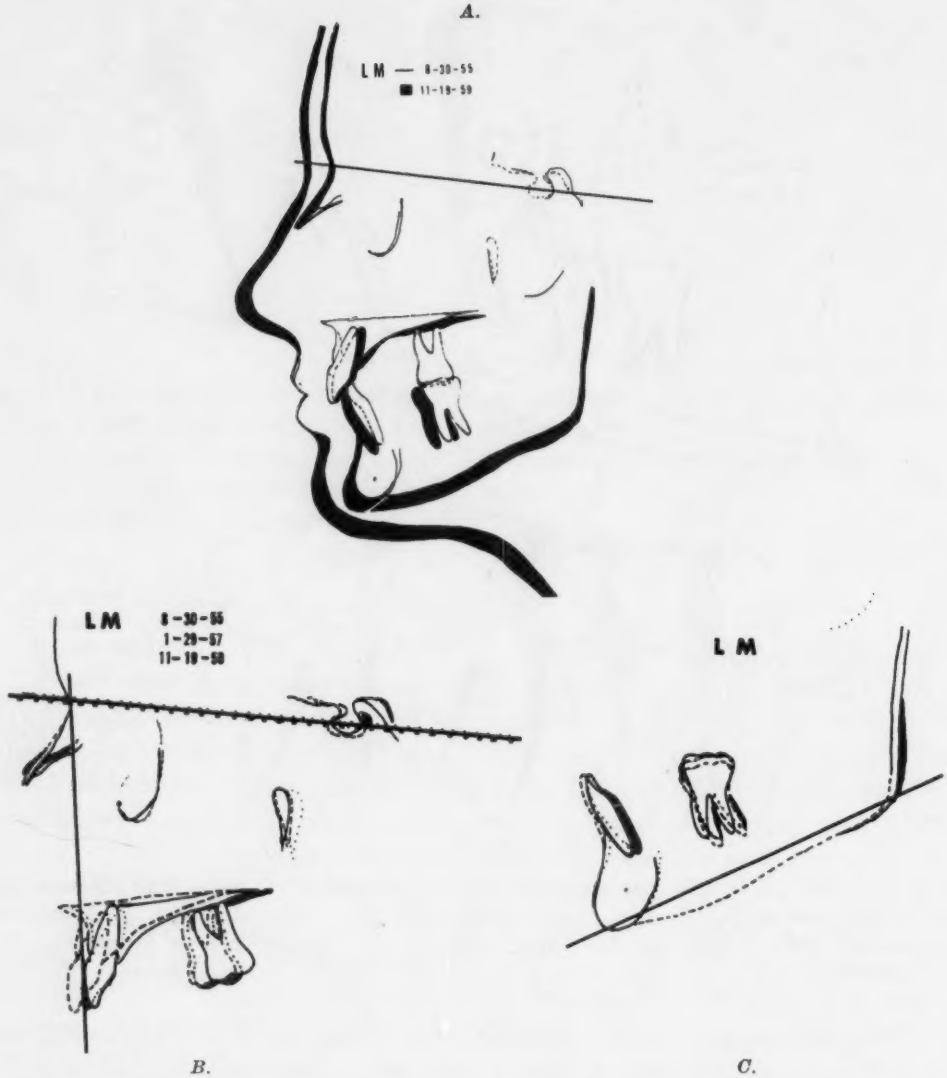

Fig. 6. Patient L. M. *A*, Tracings superimposed on SN plane and registered on sella. Note the forward mandibular growth.

B, Tracings superimposed on NA plane and registered on palatal plane, showing forward movement of molar crown with center of rotation near apex. This tooth was left in tipped-back posture.

C, Tracings superimposed on GoGn plane and registered on D point. Note that the lower incisor has not moved forward from Jan. 29, 1957, to Nov. 19, 1959. In this case it stayed very permanently in the position where it was placed at the end of treatment, even though the lower molar tipped forward in the postretention tracing.

case of Patient C. H. (Fig. 4) the lower buccal segments were moved distally, and in uprighting themselves the apices of the tipped-back lower second molars actually moved distally 2 mm. further than they were at the end of active treatment or had ever been previously. Also, the distal cusp of the lower molar went up and contacted the upper second molar in the postretention period. Judging

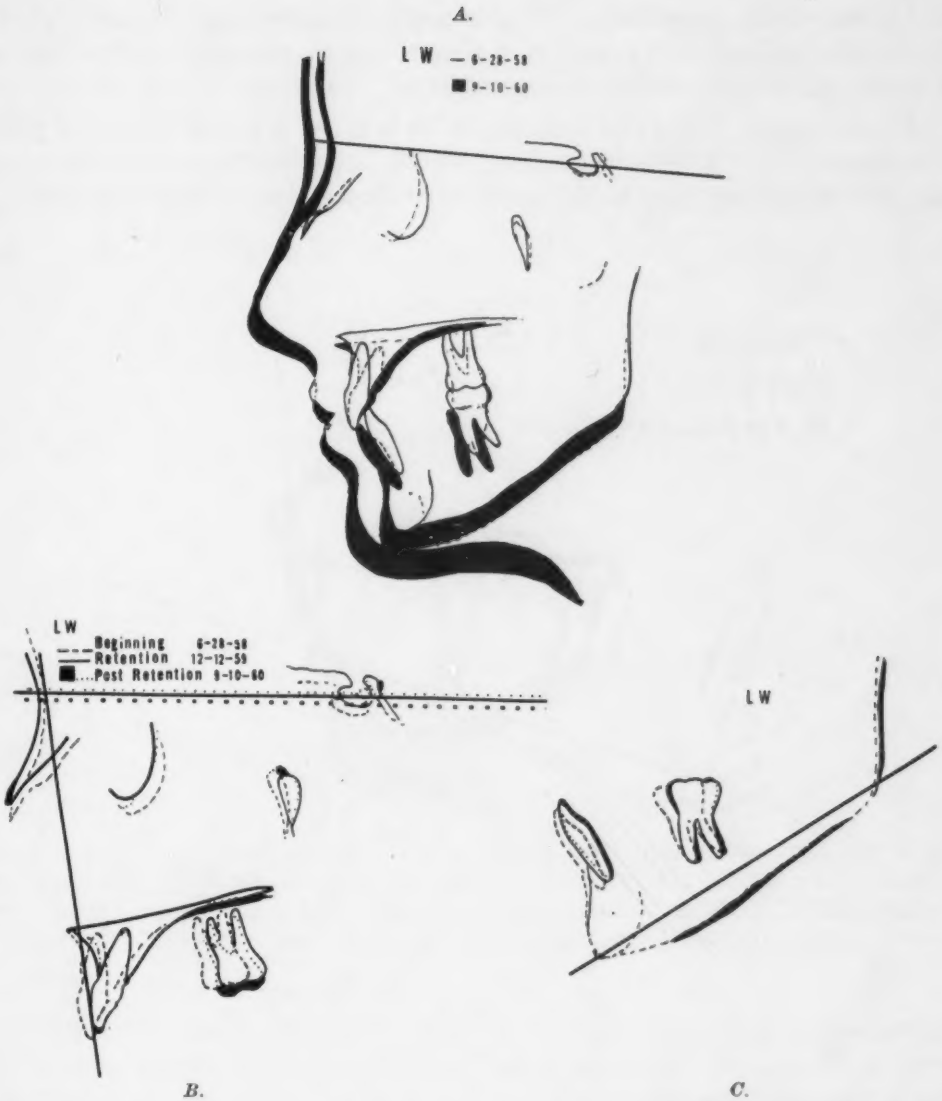

Fig. 7. Patient L. W. *A*, Tracings superimposed on SN plane and registered on sella. Note over-all growth.

B, Tracings superimposed on NA plane and registered on anterior nasal spine and palatal plane. Note change in position of point A.

C, Tracings superimposed on GoGn plane and registered on point D. Note that the lower incisor stays in position and does not move forward, even though the lower molar moved 1.5 mm. forward in uprighting from its tipped-back position. Also note that the lower molar root moved distally in this case.

from the results of treatment seen in other cases several years out of retention, these teeth will remain in their upright position and not drift mesially (Figs. 6 and 7). So far there has been no such tendency in the case of Patient C. H.

Holdaway had noted that in properly treated cases the position of the lower incisor in relation to the NB plane is very permanent and that it will

usually stay in the position in which it is placed at the end of treatment. This observation was noticeably true in the cases observed in this study. The case of Patient L. M. (Fig. 6) is a typical example.

In attempting to acquire arch length, it is much better to treat the patient at a younger age. If the case can be treated before the premolars and cuspids erupt, there is much greater likelihood that this arch length will be permanently

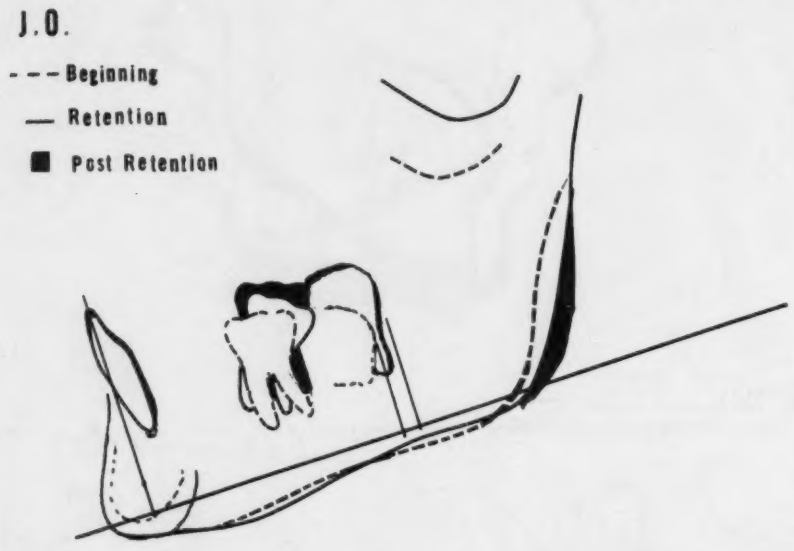

Fig. 8. The lower second molar was caused to erupt 2.5 mm. distal to its former position in relation to a vertical line from the GoGn plane. Note that the lower molar has tipped forward some in reverting from its tipped-back position, but the incisor has not moved forward. This is the result of taking up band space and slight expansion in function.

increased a slight amount by expansion or by uprighting of mesially tipped lower buccal segments. In the upper arch, the Kloehn type of cervical gear is very effective for distal movement.

By tipping the lower teeth back, the orthodontist can sometimes force the premolars and cuspids to erupt in a more distal position.

Fig. 8 illustrates a nonextraction case in which the lower buccal segments of Patient J. O. were severely tipped back with Class III elastic ligatures acting on tipped brackets.[9] Then it was treated with strong Class II elastic force. At the time the treatment was started, the lower second molars had not erupted. Only the crown portion was formed. At the time of retention, the lower first molars were still in a tipped-back position. Two years later, superimposition on the lower border and symphysis showed that the lower second molar was 2.5 mm. posterior to its most previously posterior position in relation to a vertical line from the GoGn plane.

Whether the upper molars are actually driven back by the Kloehn type of cervical gear or just inhibited from their normal forward and downward

growth[12] is a somewhat controversial matter. Regardless of controversy, however, the same things are accomplished in a growing patient: The Class II molar relationship is corrected. In a Class I crowded case, the crowding is corrected.

PITFALLS OF NONEXTRACTION TREATMENT. A case in which too much optimism in treatment planning has assured its failure is that of Patient D. D. (Fig. 9).

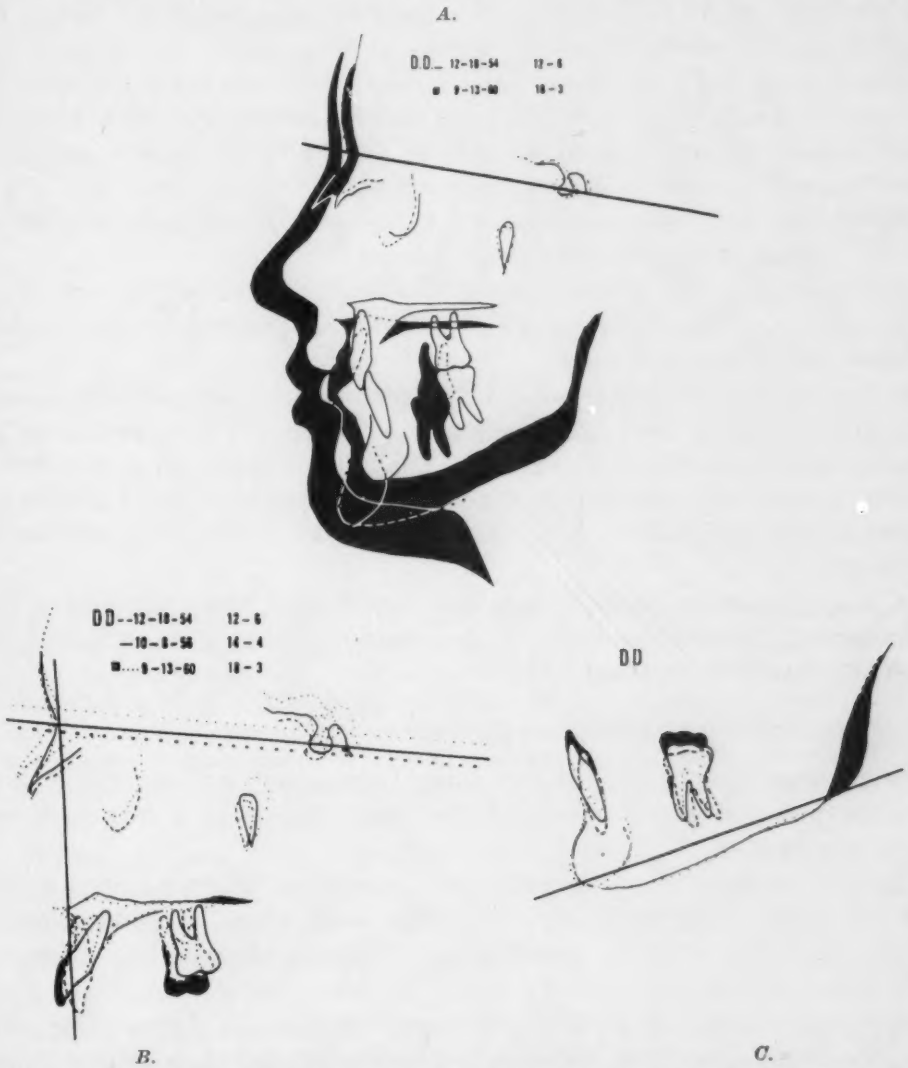

Fig. 9. Patient D. D. *A*, Tracings showing over-all growth. Superimposed on SN plane and registered on sella. Note forward mandibular growth.

B, Tracings superimposed on NA plane and registered on palatal plane. Point A moved distally with distal movements of apices of incisor roots.

C, In this discrepancy case the unfortunate decision was made to expand lower incisors forward. The lower teeth are in a position of instability, even though the posterior occlusion is acceptable. Class III elastics and anchorage preparation were not used. Note that point B moved distally as the lower incisor moved forward.

There was a 5 mm. deficiency in lower arch length plus a 4 mm. mesial drift of a lower first molar due to premature loss of a lower deciduous molar. The maxillary right cuspid was unerupted, with no space available. The upper right lateral incisor was lingually locked.

In the treatment of this case, the upper anterior teeth were very carefully torqued back, causing the base in the maxillary arch to diminish—just what we do not want when we are trying to occlude the maxillary arch with a full lower arch. In other words, I almost made a Class III case out of a Class II case. The SNA angle decreased from 82 degrees to 80 degrees. In the lower arch, instead of using Class III elastic distal force to drive the lower segments back and recover arch length which had been lost by mesial drift of a lower first molar, I made the unfortunate decision to pit the lower incisors against the drifted lower molar with coil springs and no outside distal force.

Four years later, this case showed a 1 degree ANB difference as compared with the original ANB difference of 3.5 degrees.

This case has a nice posterior occlusion, but the lower incisors are off base and obviously unstable. The upper right lateral incisor shows a strong tendency to become lingually locked again.

In retrospect, I should never have attempted anterior expansion in the lower arch. Any attempt at recovering lower arch length should have been made with a continuous intermaxillary distal force. The upper anterior teeth should never have had lingual root torque; rather, a mild labial expansion and a Kloehn type of cervical gear should have been used to drive the molars back and increase arch length.

A better treatment plan for this case would have been extraction of four premolars and conservative anchorage preparation. There was insufficient bony base in either arch to accommodate the teeth.

PERMANENCE OF THE RETENTION POSITION OF THE LOWER INCISOR

In the treated cases studied, one thing became very evident, namely, that the lower incisor correctly positioned over bony base is in a very permanent position[5] (Figs. 6, *C*, 7, *C*, and 8).

Even in the cases in which the lower molars were violently tipped distally, the lower incisor held its position while the lower buccal segments were uprighting themselves with the assistance of the forces of occlusion (Figs. 8, 4, and 7, *C*).

A partial explanation of this phenomenon is that the forces of occlusion, pounding on the mesial cusps of a tipped-back lower molar, cause the roots to go distally (Fig. 10). The lower teeth are restrained from forward movement by the interlocking cusps of the stabile maxillary arch and the maxillary anterior teeth, which are in front of them. The lower anterior teeth are locked together as a unit by a cuspid-to-cuspid lingual arch during the retention period.

From this explanation, one can see why it is very desirable to have the lower arch tipped back slightly in Class II cases and also why the maxillary teeth should be moved back root first.

It must be mentioned that these cases were completely treated before retention, with the root apices of the maxillary posterior teeth moved distally as much as the crowns. No second-order bends were used in the upper arch wire except to keep the teeth in a vertical position during Class III anchorage preparation. In other words, the upper posterior teeth were moved back bodily. The upper incisors and canines were moved distally root first, with careful attention given to lingual root torque.

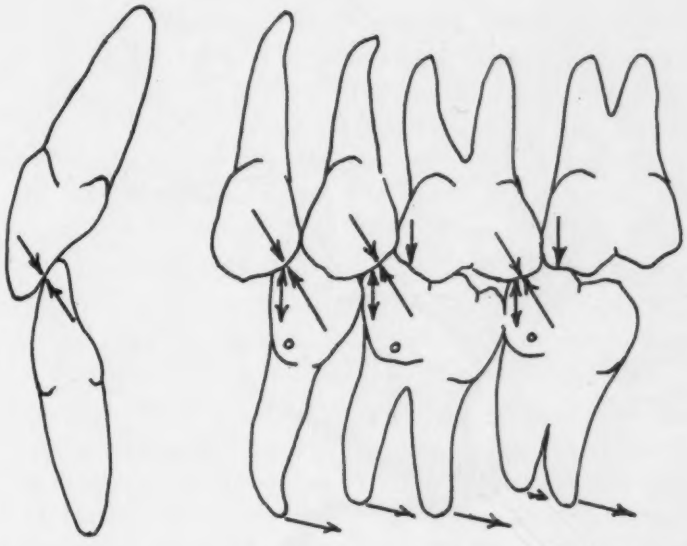

Fig. 10. Drawing showing how the forces of occlusion will upright tipped-back lower teeth by pounding on the mesial cusps. The fulcrum is at about the dentoenamel junction in the region of the mesiobuccal cusp. The teeth of the lower arch are restrained from forward movement by the cusps of the maxillary arch and the maxillary anterior teeth which are in front of them. The lower anterior teeth are locked as a unit by a cuspid-to-cuspid lingual arch during the retention period. It is very important to leave the lingual arch in place for a long period when much anterior mandibular growth is anticipated.

Holdaway,[5] in a study of headfilms of treated cases, observed that distally tipped maxillary posterior teeth almost always return to a vertical position. There is a forward migration of the crown, the center of rotation for this movement being at the root apex (Fig. 6, *B*).

A case finished with the upper posterior teeth in tipped-back positions must be retained with a distal force upon the entire upper arch. They must be so retained until such time as the maxillary molar teeth have returned to their normal anterior-posterior axial inclinations, or a forward positioning of the entire maxillary denture will result.

TEETH CAN BE MOVED DISTALLY THROUGH THE "LEEWAY" SPACE

This study convinces me that mesial drift of the lower first molar is not inevitable in the transition from the mixed to the permanent dentition and that, by the use of constant distal force from Class III elastics, the teeth mesial to the

first molar can be moved distally through the leeway space (Fig. 11). It will stay very permanently in this relationship as long as the upper buccal teeth have been moved distally, root first, by means of extraoral anchorage into a Class I or cusp-in-groove relationship with the lower first molar. The root apices of the upper first molar must be moved far enough distally to place the tooth in a vertical or upright posture. Locking of distal cusps of the upper first molar in the distal sulcus of a distally tipped lower first molar is usually an indication of stability.[6]

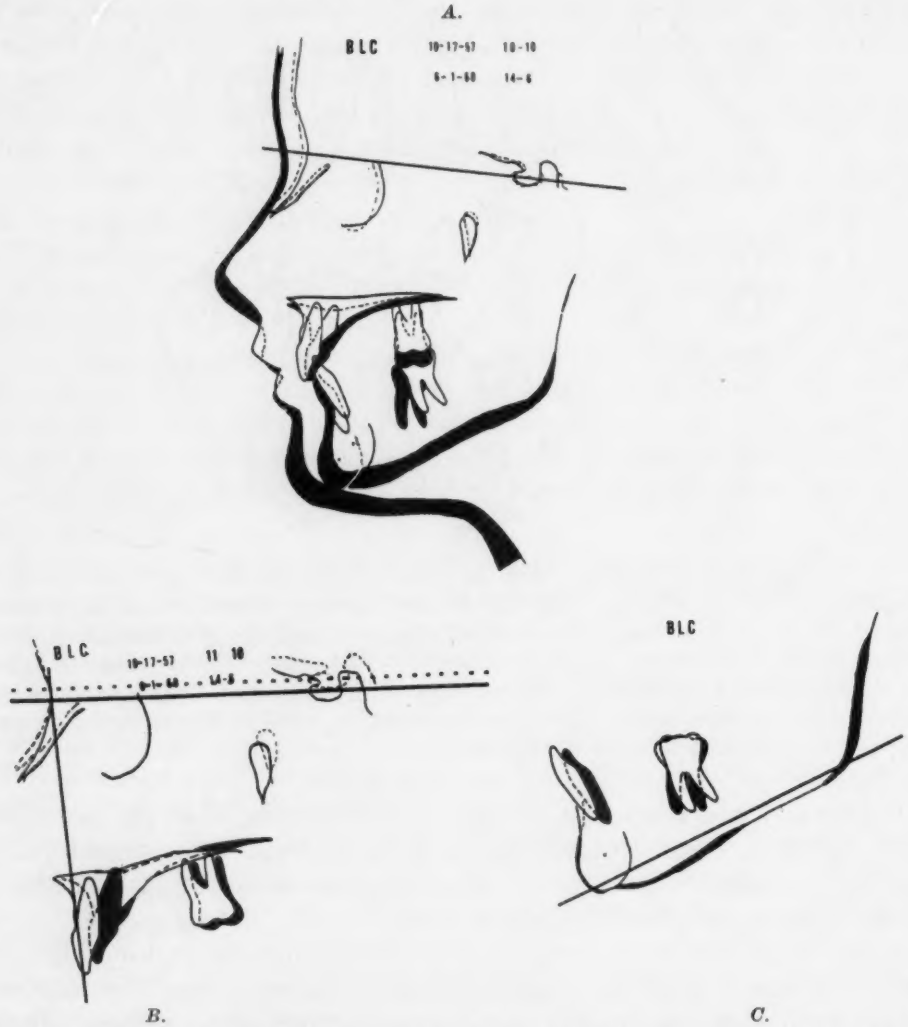

Fig. 11. Patient B. L. C. *A*, Tracings superimposed on SN plane and registered on sella, showing over-all growth from 10 years 10 months to 14 years 6 months of age.

B, Tracings superimposed on NA plane and registered on anterior nasal spine, showing movement of maxillary teeth.

C, This case illustrates that the teeth anterior to the first molar can be moved distally through the "leeway" space. Note that the lower molar contact point has moved distally only 2 mm., while the lower incisors have moved 4 mm. distally. Superimposed on GoGn plane and registered at point D.

I observed that this frequently happened as the lower first molar was returned from its tipped-back position by the forces of occlusion.

Any bodily distal driving[3] of the lower buccal teeth is exceedingly difficult, and anyone who glibly states that this movement is possible with simple procedures should study his headfilms more closely. In the cases in which bodily distal movement of lower teeth actually took place, this was done with a great deal of effort, and time alone will tell whether the positions of these teeth are permanent.[1]

In saying that the mandibular first permanent molars always come forward in transition from the mixed to the permanent dentition, Nance was merely stating a fact, namely, that the lower first permanent molar is obliged to erupt distally to its normal cusp-in-groove relationship with the upper first molar because of the difference in size between the upper and lower deciduous molars and cuspids. As a theoretical average, the lower molar would come forward 1.7 mm. while the upper one would drift only 0.9 mm.

Nance was not considering the possibility that the lower first molars could be mechanically tipped back enough to position the lower incisors distally over the base and that the upper molars could be moved by extraoral force and Class II elastics into a cusp-in-groove relationship with this tipped-back lower molar.

It seems to me that an increase in arch length between the symphysis and the lower first permanent molar beyond a mere uprighting of teeth and slight expansion is very likely to be fraught with disappointment and that one would be wise, indeed, to heed Nance's advice regarding our limitations in this respect.

One noticeable phenomenon in the study of headfilms is that it is very difficult to position point B anteriorly in relation to pogonion.[13] In the case of Patient D. D. (Fig. 9), for example, point B moved distally as the lower incisors moved forward.

MEANS USED TO GAIN ARCH LENGTH

Study of the stable treated nonextraction cases reveals that *all* of the means[2] of aligning teeth in nonrotated positions were used, with the exception of labial movement of mandibular incisors. (All mandibular incisors were kept behind their malocclusion positions.) However, all the means of gaining arch length were used to a very *minor* degree.

Methods of increasing mandibular arch length[2] are as follows:

1. Distal movement of mandibular posterior teeth.
2. Uprighting of mandibular posterior teeth which have actually tipped forward.
3. Labial movement of mandibular incisors.
4. Buccal expansion.
5. Rotation of mandibular molars and premolars.

I observed that several of the cases actually widened 1 or 2 mm. in the molar and second premolar region in the functional postretention period, even though

there was some lateral expansion from beginning to retention. This indicates to me that possibly more expansion in the beginning would have simplified treatment in these particular cases.

BUCCAL EXPANSION

One of the very mysterious means of increasing arch length is buccal expansion. How much expansion can be accomplished with the expectation of its being held permanently?

Howes[14] pointed out:

> When the basal arches are wide enough, but the coronal arches are not, the coronal arches can be expanded; and, if the environmental forces which would cause relapse are eventually eliminated by further growth, elimination of habits, or a new balance of muscular forces, then the expansion will be maintained. This might apply to about 20 per cent of our cases.

It was suggested[14] that, by moving the teeth distally into a wider part of the arch, greater arch length could be gained.

In reviewing photographs of models in the articles by Howes[14] regarding arch width in the premolar region, I noted that the cases which he considered

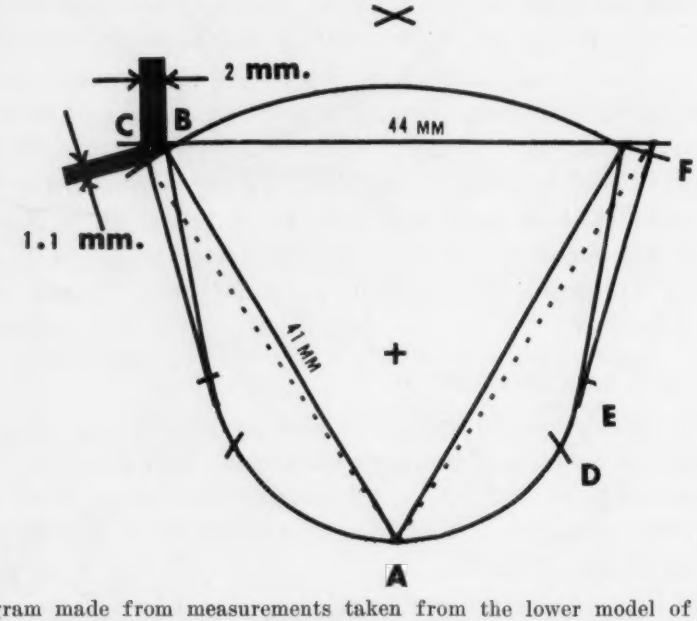

Fig. 12. Diagram made from measurements taken from the lower model of Patient L. W. The inside measurement *AB* is taken from between the lower incisors at the distoincisal angle to the mesial contact point of the second molar. The line *AD* represents the distance from between the central incisors to the distal side of the cuspid and is used as the radius of the front part of the diagram. The line *AE* represents the distance to the distal portion of the first premolar. The distance across the arch from the mesial contact points of the second molar is 44 mm.

If, in this hypothetical case, the second molar is moved laterally 2 mm., the inside measurement will increase only 1.1 mm. *(AC)*.

The increase in the line through the contact points *(AEF)* will increase only approximately 0.7 mm. This shows that lateral expansion at the distal portion of the arch accounts for very little increase in arch length.

to be successful were expanded a great deal more in the first and second molar region than through the area of first premolars and canines.

In many of our early cases treated by expansion, insufficient efforts were made to maintain the full length of the mandibular arch, and as the mandibular arches became wider they became shorter. Hence, there was no net gain for the irregular teeth, and often there was a net loss of space, if intermaxially Class II force had been used.[14]

In the diagram shown in Fig. 12 a divider was used in construction; one point was placed between the lower incisor teeth at the linguo-incisal edge and the other point was placed at the mesial marginal ridge of the second lower molar in the center of the tooth (or down the central groove).

The distance from this spot at the contact point of one molar to the opposite molar was measured. These measurements were taken for the purpose of constructing a hypothetical case to demonstrate how much arch length increase would take place with each millimeter of lateral movement at this molar position. It was mathematically calculated that the straight inside distance from the molar position to the incisor would increase 1.1 mm. for each 2 mm. of lateral movement. The arch length at the contact points of the hypothetical arch would increase only 0.7 mm. However, an increase in arch length due to lateral expansion in the cuspid area would be quite a different story.

PITFALLS OF EARLY NONEXTRACTION TREATMENT

Early nonextraction treatment in the mixed dentition in borderline cases consists of remodeling the anterior portion of the arch. The length of this portion of the dental arch is a known factor. What we are doing is borrowing from the distal portion of the alveolar trough, which at this time is an unknown quantity. We will not know for several years how much each patient is to grow in this area. As we tip the first molars back, we impact the second and third molars.

We must rely upon clinical judgment in estimating the patient's potential in this regard. The case of Patient R. C. (Fig. 13) illustrates the tremendous growth that will sometimes occur during and after treatment.

One of the dangers of early nonextraction treatment, especially in growing patients, is overtreatment of upper incisors.

In Class II or crowded cases which have normal or ideal[7] apical base relationships before treatment or which tend toward very straight profiles, it is imperative that cervical gear be used to correct the molar relationships, but caution should be used in torquing back the upper incisor roots early in treatment.

In this type of case it is foolhardy to attempt to increase lower arch length beyond a mere uprighting of the lower first permanent molars or slight expansion. It will be disastrous to make room for lower teeth in a case of maxillary deficiency if the patient has a forward mandibular growth.

This type of problem is exemplified by the case of Patient M. C. (Fig. 14). I felt very good about this case until I took follow-up headfilms. The mother

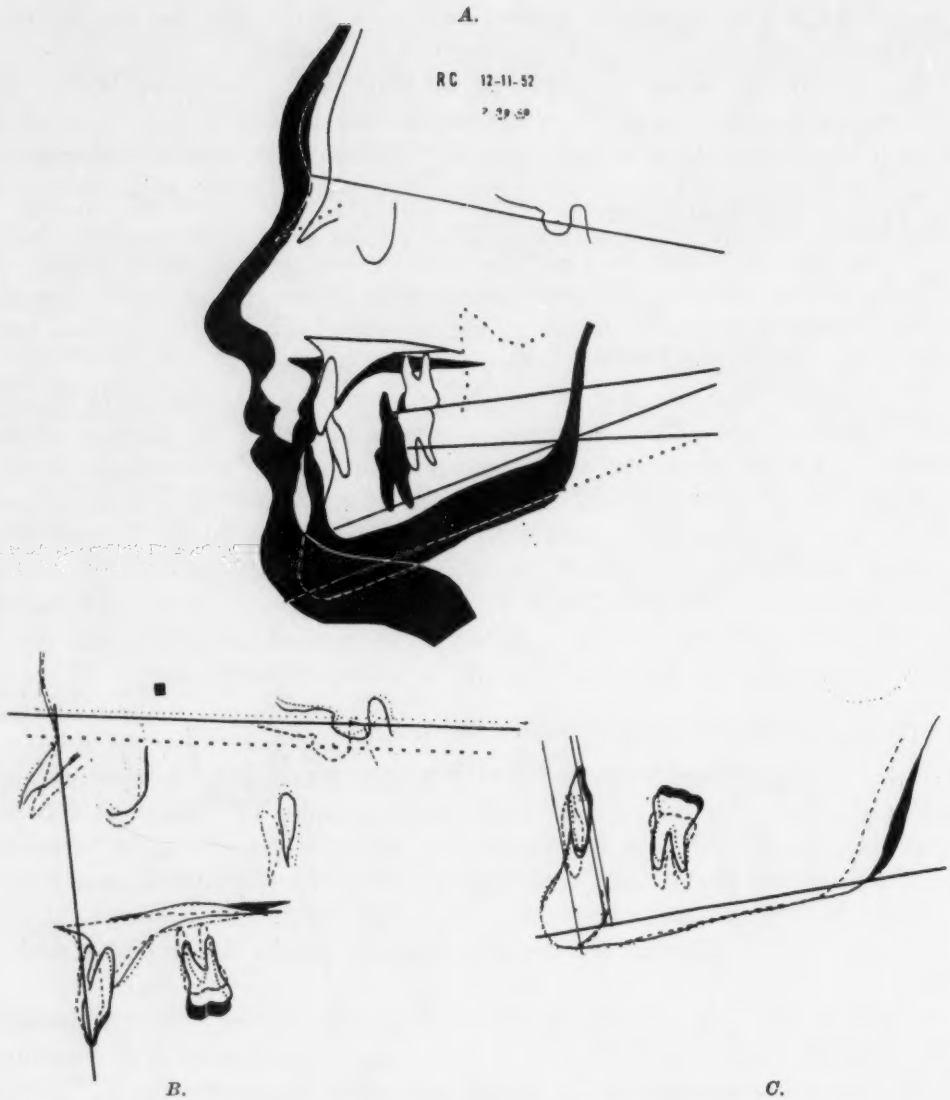

Fig. 13. Patient R. C. *A,* Tracings superimposed on SN plane and registered at S. Note the tremendous mandibular growth from beginning to postretention tracing.

B, Tracings superimposed on NA plane and registered at anterior nasal spine show movement of maxillary teeth.

C, Tracings with mandible superimposed on D point and GoGn plane, showing movement of lower teeth. Note that the lower incisors moved an additional 2 mm. distally in postretention period from 16½ to 19⅓ years of age. They were set back on the body of the mandible by mandibular growth and the restraining action of maxillary teeth.

confided that the child was beginning to look "toothy" and wondered if her teeth protruded. It is now obvious that this case would have been more successfully treated with extraction of four second premolars and deliberate sacrifice of anchorage. There was insufficient apical base in both dental arches. There was a 6 mm. deficiency in the lower arch (not counting the 1.7 mm. which some may want to add to the deficiency).

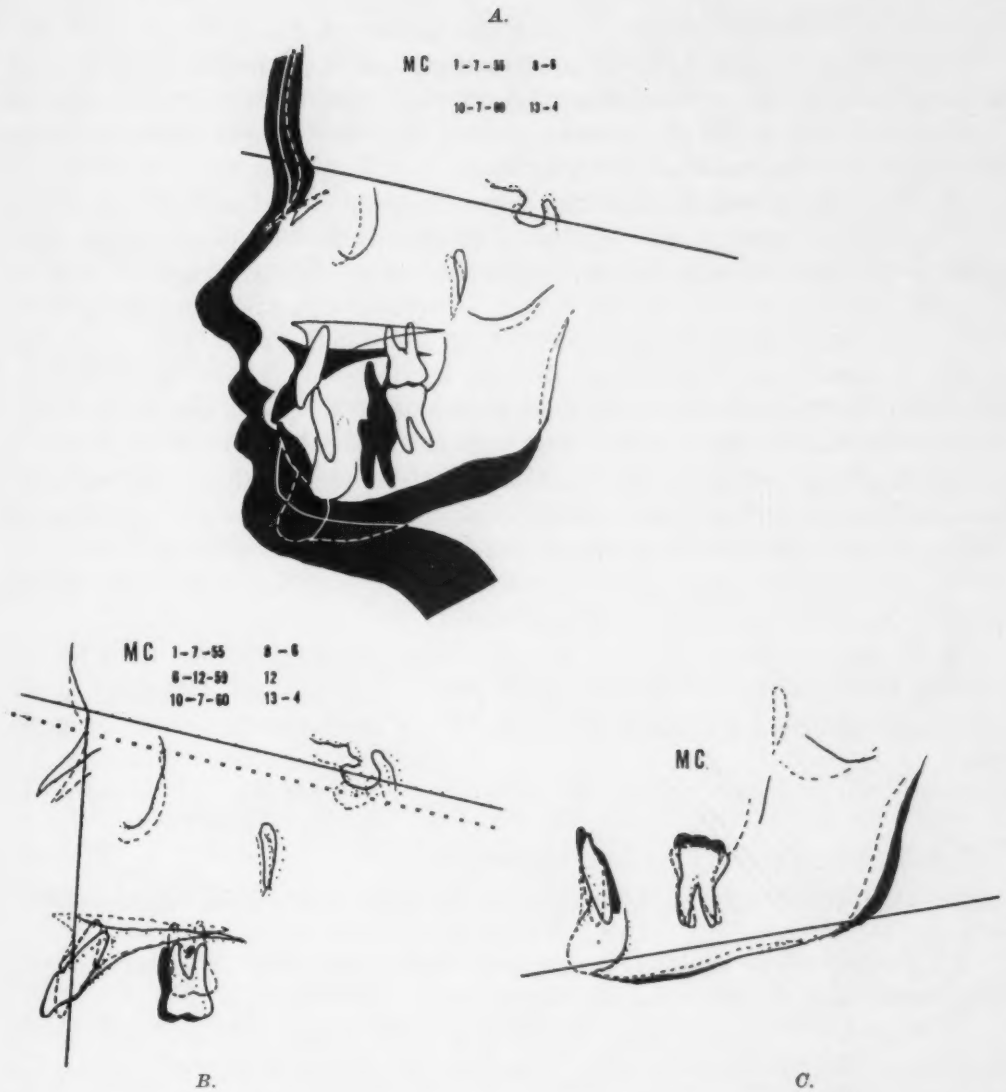

Fig. 14. Patient M. C. *A*, Tracings showing over-all growth. Note forward mandibular growth and end-to-end bite. Superimposed on SN plane and registered on sella.

B, Tracings registered on NA plane and registered at anterior nasal spine. Note excessive lingual positioning of upper incisor root due to anterior growth of the mandible.

C, Tracings superimposed on GoGn plane and registered at point D. Note that lower incisors have moved forward in postretention tracing.

It will be interesting to note that 3 mm. expansion across the first molar region was held. It is quite obvious, however, that the cuspid expansion is doomed as a result of being moved forward by the forces of occlusion as the lower buccal segments uprighted, involving a 3 mm. forward migration of crowns. I feel that this happened because there was no restraining influence of the upper arch, as there is when the anterior overbite stays deep. It appears that the forward growth of the mandible, which had too much tooth structure to be stable in its own apical base, grew ahead of the maxilla.

VARIABILITY OF GROWTH

The case of Patient R. C. (Fig. 13) shows how unpredictable growth[15] can be. In this case there was a 3 mm. total anterior mandibular growth in relation to sella, registered on SN plane, and a vertical growth of 20 mm. from beginning of treatment to the postretention headfilm.

Between the ages of 12 and 16½ years, the patient had only 1 mm. of forward mandibular growth and 14 mm. of downward growth. Between the ages of 16½ and 19⅓ years, he had an additional 2 mm. of anterior growth and an additional vertical growth of only 6 mm. During the postretention period, the mesial edge of the lower incisor was back an additional 2 mm. from the D line (a line vertical to the GoGn plane through the D point[7]), illustrating that the lower teeth are sometimes set back on the body of the mandible by growth.[16] This case would also indicate that growth is influenced by treatment.

In diagnosis and treatment planning, the use of cephalometric headfilms, intraoral x-rays, photographs, careful analysis, and measurement of casts, as well as clinical examination of the patient, should be made. Observation of the parents may produce clues as to the potential growth of the patient. However, we must be careful not to jump to conclusions.

Referring to parents, Wylie[16] states: "You might learn something by appraising their stature and profiles, but if you think a cursory inspection of the parents constitutes a genetic survey, you have grossly oversimplified the problem."

SUMMARY

The following points have been mentioned:

1. Considerable space can be obtained by distal tipping of tipped-forward lower buccal teeth.
2. It is possible to tip lower buccal teeth distally and have them upright with roots distal to where they had previously been.
3. In properly treated cases, tipped-back lower molars will be uprighted in function by the forces of occlusion, with the roots going distally more than the crown moves forward.
4. When a tip-back force is exerted by an arch wire, there must be a constant distal force to avoid anterior displacement of roots.
5. In many crowded cases, it is not uncommon to discover that the lower molars are tipped mesially.
6. When it is anticipated that Class II elastics will be used, it is very desirable that they be preceded by Class III elastics to prevent anterior cant of the occlusal plane.[5]
7. In deficiency cases with low ANB angles, we must be careful not to move point A distally with lingual root torque while at the same time attempting to make room for teeth in the lower arch.
8. A stable upper arch exerts a restraining influence on tipped-back lower teeth being uprighted by the forces of occlusion.
9. A mesial drift of molars is not inevitable in transition from a mixed to a permanent dentition.

10. Lower teeth can be mechanically moved distally through the "leeway" space with likelihood of their staying there.

11. The anterior-posterior position of the lower incisors in stable cases is one of the most permanent of all tooth positions.

12. Lateral expansion of the second molar position causes very little increase in arch length. The amount of increase of the inside measurement[1] is not the same as the increase at the contact point.

13. The basic limitations of treatment should always be respected in treatment planning.

CONCLUSION

In attempting nonextraction orthodontic treatment, one must be careful to conserve anchorage if the patient is to be left with good facial balance at the conclusion of the treatment. The basic limitations[1] are always lurking to upset the most carefully laid plans, and these limitations must be respected.

Slight deficiencies in arch length can be overcome by judicious use of potential growth and distal movement of buccal teeth.

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60 South 4th East St.

Panel discussion of essays presented at the scientific session of the American Association of Orthodontists

AN INTERESTING feature of the 1961 meeting of the American Association of Orthodontists in Denver, Colorado, was a panel discussion of essays presented at the scientific session. The moderator for this discussion, which took place on April 18, was Wendell L. Wylie, president of the American Board of Orthodontics and Professor of Orthodontics at the University of California School of Dentistry. The panel was made up of the following official discussors:

Melvin I. Cohen, B.S., D.M.D.
Assistant Clinical Professor of Orthodontics, Harvard School of Dental Medicine

Egil P. Harvold, L.D.S., Ph.D.
Professor and Chairman, Orthodontic Department, University of Toronto

J. Rodney Mathews, A.B., M.A., D.D.S.
Assistant Professor, Division of Orthodontics, University of California School of Dentistry

Robert E. Moyers, D.D.S., Ph.D.
Professor of Dentistry (Orthodontics), School of Dentistry, University of Michigan, Ann Arbor, Michigan

Samuel Pruzansky, D.D.S., M.S.
Associate Director, Cleft Palate Clinic and Training Program, Research and Educational Hospitals; Associate Professor of Orthodontics, University of Illinois

C. Willett Asling, M.D., Ph.D.
Professor of Anatomy and Co-Chairman, Department of Anatomy and Physiology, University of California, Berkeley, California

Louis J. Baume, D.M.D., M.S.
Professor of Dental Medicine, University of Geneva, Switzerland

John F. Crigler, Jr., M.D.
Associate in Pediatrics, Harvard Medical School, and Associate Physician, The Children's Hospital, Medical Center, Boston, Massachusetts
Henry J. Ralston, Ph.D.
Department of Physiological Sciences, College of Physicians and Surgeons, and the Biomechanics Laboratory, University of California Medical Center, San Francisco

A transcript of the session follows.

William R. Humphrey: We have with us today a very distinguished panel. I have been in the American Association of Orthodontists since the days when we had 166 members, so I feel that I am competent to speak on the quality and eminence of those whom we are privileged to hear this morning.

The moderator of this panel, one of our own members, is exceptionally competent for the task. He is known to all orthodontists throughout the world as a lecturer, research worker, and practitioner in orthodontics. It is my pleasure now to turn this panel over to Dr. Wendell L. Wylie, the moderator.

Dr. Wylie: I propose that we discuss the papers in the order in which they were presented. In the case of each paper, one of the panel members will make an opening statement. I shall then ask others on the platform, whether they be essayists or discussors, to discuss the paper. Cards have been provided for the audience to write their questions, and there are two men who will bring the cards up to the platform.

We shall start with Dr. Mathews, who will open the discussion of Dr. Baume's paper.

Dr. Mathews: Thank you, Dr. Wylie. Being up here with this outstanding group of panelists and discussors, I am tempted to take to heart some words of Mark Twain. You will recall that he said there are certain times when it might be better to keep your mouth shut and appear stupid than to open it and remove all doubt.

Dr. Baume's paper sounded the keynote for this scientific session, and I was very glad to have the privilege of hearing it. He has reminded us again, as have the other panelists, of the importance of considering our work in the terms in which it really should be considered, those of a biologic system. After all, a biologic system is what we are dealing with. Too many times, as clinicians, we become so concerned with the mechanical problem at hand that we tend, in spite of ourselves, to lose sight of the other aspect, the importance of recognizing that we deal with living tissue.

Again, thinking as clinicians and as practicing orthodontists, we come to these meetings and ask ourselves as we are leaving, or possibly before we have left: "What have I learned or what can I take home to use in the office?" We are all looking for this. If it is a meeting at which assorted little gadgets are shown, we are almost certain to find something to take home.

In this type of meeting, however, where we have basic science under consideration for the most part, sometimes we might feel discouraged and think that there is nothing to take home. Perhaps there is not anything for today; on the

other hand, perhaps there is something that we can use tomorrow, and it is in this particular area that I would like to direct attention to some aspects of what Dr. Baume had to say.

He pointed up very well the dual origin of bone. He likewise pointed up very nicely the relative stability of cartilage, whether it is modified in one form or the other in relation to the environmental stress that is put upon it.

Dr. Baume showed a case of micrognathia in which traction was placed on the chin and the jaw grew. When we go home and remember this case we may have to ask ourselves: "Did that jaw grow because of Dr. Baume or in spite of him? We were fortunate later in the afternoon to see another case of micrognathia in which the jaw grew well, and this case did not have traction on the chin.

I bring this up because I think that, as practicing orthodontists, we like to tell ourselves that when, in treating a patient, we have used a certain type of mechanics (for instance, some type of Class two elastics), we have thereby stimulated growth of the jaw, and especially if the jaw happens to have grown. I think we also tell ourselves sometimes that if the jaw did not grow very well we can disclaim this because the patient did not wear the rubber bands. You see how it goes; this is only human. We all tend to do the same thing.

One of the basic things that should enter into further discussion today is the following question: "Are we yet in a position to be able to say that we can differentially inhibit growth? Can we do this at all? Where do we stand? (By "we" I mean not myself or us as clinicians but these panelists who are really concerned with basic science.) These are things that we might consider.

Dr. Wylie: Thank you, Dr. Mathews. Dr. Baume, do you have any comments before we go on to the other discussors?

Dr. Baume: Yes. This is unrehearsed, and I have to be very careful of what I am going to say because my paper yesterday was rehearsed and you saw what I had to say when the slides were explained.

First I would like to say something which is very dear to my heart when we talk about biology. Then I would like to mention one thing to you orthodontists: The first aim and the basis of orthodontics certainly is to master the techniques. You have to treat and you have to master the technique of doing that, and you do that better than anybody in the world. I know that; I have seen it, and I am thoroughly convinced. I would like to have your ability, but I do not have it. First we want to know what we are doing. We want to analyze our cases. Then when we go into biology, we do something for the future. It is the ideal goal that we should one day be able to prevent and to interpret early malocclusion and dentofacial anomalies.

Last night I attended a meeting on orthodontics in public health. It occurred to me more than ever how important it is that if the orthodontist wants now to go into public health, or if he is forced to go into that field, and care for everybody, it would be impossible to do it in terms of therapy; we can do it only in terms of prevention or early interception. This is why I think biology plays

such an important part. It is for the future, and we have to get to that aim. At least we from the universities and we research people do. I hope I stressed this point, which is a most important one.

In answer to the questions raised by Dr. Mathews, may I say that two years age, when we had the case of micrognathia, I got the papers of Dr. Pruzansky, who has surveyed all those untreated cases of micrognathia, and I saw that there is certainly a growth tendency in those cases, which Dr. Pruzansky is better qualified to discuss, and I knew that our evidence was not complete.

From one case we have learned two things, and they are important. We have learned that we can interfere and that we can, as you put it, differentially intercept some growth tendency. Transformation of the fossa was something quite new to us. If you look at normal cases with normal development, you do not see transformation. This one case at least showed us that micrognathia occurs not only in monkeys but also in human beings.

We are able to influence the cranial complex and not merely, as we had always thought, the alveolar processes. We can further influence, in some way, the condyles and even the temporal bone. This is my statement for the beginning.

Dr. Wylie: Dr. Pruzansky, would you like to comment on this trend of thought?

Dr. Pruzansky: I was privileged and delighted to have an advance copy of Dr. Baume's paper. It is a manuscript to which I shall have occasion to refer for many years. I had never met Dr. Baume before, though I have corresponded with him. I have a hunch that he played a prank on us. He made some statements toward the end of his paper which were designed, I suspect, to provoke. For example, one could draw certain inferences that bite plane therapy and other forms of therapy can grow condyles. I suspect that Dr. Baume is too good a scientist to really mean that; in fact, in his paper he points out that by observation we perceive a natural phenomenon and by experiment we force it to reveal its principles. It would be undignified and unrewarding to argue between different schools of treatment.

There are at least two issues here. First, is there a scientific basis for drawing certain conclusions that bite planes or traction can produce growth of the condyle? Let us begin, first of all, with the Robin syndrome in a child who is born with a very small mandible. The child cannot breathe, he cannot feed, and he is in a poor metabolic state. He is not growing, and his case is not comparable to a case of Class II, Division 1 malocclusion. When you take such an infant and put him in an adequate metabolic climate, he grows like nothing you have seen before; it is a kind of release phenomenon. Experimental biologists have seen this in growing young animals that are deprived of some metabolic factors. If that factor is then restored so that the animal can grow again, for a short period of time the experimental animal will show an increment of growth far greater than normal. It may never catch up to the normal, but for the moment, because of this release, it shows a sudden spurt.

I think we have documented this. Now, there has been no adequate controlled study to point out what the use of traction or any other procedure will do to

stimulate the mandible. I hope that no one is this audience nor any of the manufacturers in the lobby will be tempted to supplant cervical anchorage with ceiling anchorage in which a chin cup is combined with traction on a pulley to the ceiling. There is no scientific basis for that.

There is another issue of scientific import here. Experiments were carried out on monkeys. I do not know that anyone has done Downs' analyses on monkeys, but I rather suspect that the monkey represents a more homogeneous facial pattern than does the human animal. What does that mean? It means what any astute clinician already knows—that the same method of therapy, with the same amount of cooperation from the patient, does not produce the same results in degree and time for different patients. We suspect that these differences are due to biological variance beyond our control—for example, mandibular plane angle, A and B relationships, the growth potential and the direction of the mandible, etc. This has already been spelled out, and one must be very cautious about extrapolating human possibilities from experiments on monkeys.

Dr. Wylie: Dr. Moyers, do you have any remarks on this?

Dr. Moyers: There are a couple of points that have not been mentioned. I enjoyed Dr. Baume's paper. I had not met him before, although we have corresponded previously. I would like to point out two or three things that were left out of his paper but must not be left out of our thinking.

First of all, we should ask ourselves: "How specific are our craniofacial standards?" Measuring the A and B angles is not the same as measuring 1 or 2 mm. of mandibular anchorage in any direction. It would not hurt to measure actual increments. Without these, there is very little that we can say. If I do not know the mean mandibular length for an 8-year-old boy, if I do not know how much all 8-year-old boys are likely to grow by the time they are 9 years of age, and if I cannot express this statistically, then I cannot say some of the things as strongly as I think Dr. Baume said them and I do not think we can deny them as strongly as our clinical practice leads us to believe.

For example, if I have an 8-year-old boy whose mandible grows 3 mm. it might be all right if I triggered this to my activator, but how many 8-year-old boys' mandibles grow 3 mm.? When I have a series of these cases that have been followed for three years and I can say when I put the activator or intermaxillary traction in that ninety-nine times out of 100 this mandible is going to grow differently than if I had not put it in, then and only then can we talk. I have strong hunches on this. They do not happen to coincide perfectly with what Dr. Baume said, but I think we need more data on this.

My second point has to do with general action. How specific is general action on a specific mandibular measurement? Is it the reaction of much general action? Or is it the reaction of general action on a specific point? We orthodontists talk as if it is the latter, but my hunch is that it might be the former. If it is the former, then there is some range of general action on mandibular length, and it is what we can do within that range that we ought to be discussing. I do not know which is correct, and I do not think it has been adequately tested as yet.

Dr. Wylie: Dr. Cohen?

Dr. Cohen: The only comment that I might make is really in the form of a question. Even in the absence of cephalometric standards for monkeys (and we need them for human beings more than we do for monkeys), I wonder what Dr. Baume's reaction would be in terms of his experiment with the bite planes. What would be the effect of this anterior positioning of the mandible (we assume that this is so, even though Dr. Moyers' criticisms are right)? I wonder what the effect would be when the soft tissues come into play in the absence of that bite plane?

Dr. Wylie: Dr. Asling, do you want to discuss this?

Dr. Asling: Perhaps just one word at least from the laboratory standpoint. Of course, we are struggling between whether or not, in the experimental manipulation, the results were possibly or quite probably associated with the mechanical forces being applied. I think there are a great many circumstances in laboratory work in which it is clearly possible that the results which Dr. Baume showed, at least considered from other areas in the skeleton, were quite possibly attributable to his experimental maneuver, and I simply cannot comment on the probability. This is really the matter at issue, I think.

Dr. Wylie: Dr. Ralston has indicated that he does not care to get mixed up in this. How about Dr. Crigler?

Dr. Crigler: I think I will stay out of this also at the moment. Dr. Baume has a number of questions to answer. I was rather interested in Dr. Pruzansky's comments upon this growth spurt that you get when you start a good nutritional regime in a patient who has been deprived, and I am aware of the fact that this seems to occur. To determine why it occurs is about as complex a problem as to define what factors we are talking about in this discussion. It seems to me that, unless you can define chronologically from the time of embryonic development on through to adult maturity, the parameters involved in the development of the various bones under consideration, it is difficult to make sure that you are talking and answering the question with the same emphasis that the other person is putting on it.

There are so many factors involved. I hope most of what I said has been heard, because I do not care to repeat it. I did feel that a better definition of parameters would be desirable. At least, as I listen to this (and I am very unfamiliar with the dentofacial terms), the thing that strikes me as I watch children grow, which I have had an opportunity to do, is that they do not grow in any even plane; they do it in spurts. I think that this is also true of animals, and maybe Dr. Asling would comment on this later.

Growth is not a continuum; it is an up-and-down thing, and it occurs in cycles. Therefore in studying a situation at a particular time, one does have to have the type of standards that Dr. Moyers has brought out for that particular age group and variation in order to know what one is accomplishing by whatever procedure he happens to be using. One would also like to know what factors are involved at that particular time.

Dr. Wylie: I shall ask Dr. Baume to round up these various comments and add some of his own.

Dr. Baume: There are lots of questions, and I think I could just start from the beginning of my lecture. Dr. Pruzansky raised the question about comparability between monkeys and human beings. This, as you saw, is treated also; that is why we try and try to get human material. Our baby was the first one. We now have some other histologic specimens that we are working up, and I think we will be ready in about six months to report on our findings.

I went to California in the hope that I would get adequate human material, but I learned that this was impossible, so I had to start from scratch. In monkeys, if we measure the dental arch, the standard deviation is small in spite of the fact that the animals have been exposed to various diseases and nutritional deficiencies. I studied monkeys, for instance, that went through the experiments for polio, and even in those monkeys the dental arch was not altered in terms of width and length when I compared them with untreated monkeys.

This intrigued me quite a bit, and it is certainly true that in monkeys we have what I would call a pure race. Unfortunately or fortunately for the orthodontist, the human race has a tremendous range of variation, and all we can do is now to look into certain trends, what is possible in the human race. I think, however, that there are for all living beings some basic principles and that these basic principles are connected. Certainly they are related to the development of the skeleton and also to various other factors, and we have now learned that the condyle is responsive to mechanical stimulation; you can no longer deny that.

I collected a series of ten different papers that have been published since 1932, and all reported the same results. As I stated in the March issue of the *Journal of Oral Pathology,* "The orthodontist has to take cognizance of these biological facts, and now he has to try and at least accept the fact that he may probably be able to interfere and to control this growth with adequate therapy."

In Europe we have a functional approach and we now have quite a series of cases that have been analyzed, according to Downs or according to the methods of Schwartz, by cephalometrics. I would like to discourage your looking into this method with cephalometrics only, because this is one point I raised. We have to know the fixed points. If you do everything from sella, you probably do not get the proper picture. I do not think that you will be able to determine now whether the fossa has been transformed or whether the condyle has grown.

A very good approach, perhaps the only one that can be used on the living, is the tomogram. I have seen the material of Dr. Ricketts. He has done a tremendous amount of work, and he has not published all of it. We looked together into his material, and again he also admitted that it is almost impossible. You measure those transformations in terms of millimeters. If you get resorptions, what can you see in your shadows is also measured in millimeters. As you know, I always say that in dentistry a millimeter is a kilometer. The same applies to those transformations, and all I can say to you is, "Just try it, and try it perhaps the other way around. If this method does not succeed, try the other one."

On my trip to Canada I was interested in the Burlington study, and I met highly qualified orthodontists who showed me cases that have been treated with removable appliances. They got just the same results that we get in Europe, so for the time being, as I told you, I do not want to go into techniques because I am not qualified. Still, this has to be kept in mind.

I think that more or less covers all the questions that have been raised by the other discussors. I would like to answer Dr. Crigler that the advantage of animal experimentation is just that we can set up this cause-and-effect sequence under controlled conditions, as we will never be able to do in human beings. We do not know anything about race and we do not know too much about the nutritional background, but all these factors can be controlled in animal experimentation. That is why I plead that, for the progress of science, we have to resort to animal experimentation.

Dr. Mathews: I would like to clear up one point by asking Dr. Baume a question again. In terms of appliance therapy (we do not need to define it, whether it is removable or fixed), do you feel that you stimulate growth, an increase in size? Do you think you stimulate growth, say, of the mandible by appliance therapy?

Dr. Baume: I would say that we direct growth. As has been said, there is a genetically predetermined range of growth, and within this range we will be able to interfere with many factors, and one of the factors is function.

You always think in terms of activators, but you have another approach, and it was started in 1918. Dr. Oren Oliver has shown and published quite a number of clinical cases in which the use of his bite planes has interfered with growth, and I think we have to look more into that, too. He starts early; probably this is the trick and, as I said, we not only have to start earlier but we have to work longer. We are used to having results in a year's time, but if you use those removable appliances you think in terms of two, three, and even four years.

Dr. Wylie: Thank you, Dr. Baume. We will move on now to a discussion of Dr. Asling's paper. Later on we may well come back to this topic, but I call now on Dr. Harvold.

Dr. Harvold: I was fortunate to have the opportunity to read Dr. Asling's paper before this meeting. When he read his paper I was enjoying the arctic spring in Chicago, and for this reason I may have included in this statement some consideration of topics which he did not get time to read to you. I should like to express my sincere gratitude to Dr. Asling, for in my entire career I cannot recall another previous occasion on which a piece of research had such a pleasing effect on my dental thinking. His investigation of organisms previous to the development of a cleft is so basic that it forms a real achievement in oral biology.

Also, the possible relation among cleft palates, chondrodystrophy, and other metabolic disturbances in rats, stress lines in monkeys, skulls and orthodontics in man may appear rather remote to you or to many of us who are strongly

clinically oriented; nevertheless, a close examination of the practice of orthodontics today reveals that experimental research is of the greatest necessity. The aim of treatment in orthodontics is to set well-aligned teeth in good functional and, if possible, esthetically pleasing occlusion.

Today it cannot be denied that orthodontics is primarily concerned with the characteristics of morphology. There are two criticisms of this approach. First, since orthodontics is directed at correcting problems of occlusion, and since occlusion is governed by both form and function, then surely to direct attention almost exclusively to the former is to consider but one side of the picture.

The inadequacy of this view is well illustrated in a case I would like to show you. I have a series of such cases. This set of teeth belonged to a woman about 50 years old. She had never received any orthodontic or periodontic treatment, and very little operative work had been done on her teeth. At this age, however, she developed pain in the temporomandibular joint, and then some equilibration was done. A few months later she found that her teeth were not coming together in the front; indeed, five years later she not only had an open-bite but she also was in severe psychologic distress. Five years later the incisors are trying to accommodate to the open-bite. The explanation appears to be that certain muscular reflexes had maintained the jaws, and hence the occlusion, up to the age of 50 years. With a change in occlusal contact of the teeth the reflex appeared, and a new and probably more convenient occlusion was established. This suggested that no individual muscular reflex can be learned and maintained through the occlusal contact of the teeth. Once the occlusion is open, then this learned pattern is discarded and a new one is established.

This brings up the question of how often and how effectively our orthodontic therapy causes muscular reflex to be established. A learned reflex clearly helps to maintain our treatment results. But if some day the reflex is abandoned, then the true nature of the problem will rear its ugly head in the form of a relapse. What I really tried to emphasize is that the basic problem in orthodontics concerns the bones and the muscles with their nerves, or form and function. The way in which the teeth come together only serves to indicate whether or not harmony is established in this system.

My second criticism is that, despite the importance of morphology, the existing attitude is one which considers morphology per se, and hence obsession with measuring angles and distances between points and establishing a variety of planes as evidence for more measurements. In truth, a study of morphology makes sense only when the characteristics and observations are given proper biologic consideration.

For this reason, I think that Dr. Asling's paper has made a strong case. Through his experimental work, Dr. Asling has clearly shown us some of the more significant factors that appear to him and what, indeed, are the effects on morphology. As far as the morphology or form is concerned, we are particularly interested in knowing the relationship between the mandible and the upper part of the facial skeleton. Such a relationship is governed by three factors: (1) genetic dependence or independence of the parts involved; (2) hormonal and

nutritional factors influencing the mechanism of growth; and (3) adaptation and/or compensation to changes in the muscular system or changes in one or the other of the jaws.

The observations of the agents presented by Dr. Asling add much to this type of inquiry. For example, we know that children with cleft palate are slightly smaller than those who do not have the defect, and it has been claimed that this is because these affected children are from the lower socioeconomic brackets. Poverty in these cases seems to be, at least in part, the causative factor that has an ill physiologic impact on the child. Now Dr. Asling has demonstrated that a reduction or underdevelopment of the facial bones should be expected in cleft palate cases and that the explanation is closely related to the primary impact.

We are all aware of the various theories on cleft palate formation. So far none has accounted for the very high incidence of cases in which mandibular premolars are missing. Further studies will show whether or not the missing premolars provide further evidence to substantiate Dr. Asling's theory. He has observed that the growth characteristics on the cranium of the rat, under certain circumstances, bear a striking resemblance to those of man. I eagerly await his observations on other malformations. Dr. Asling has discussed the hormone emphasis and its effect on bone, and the similarity between animal experiments and clinical observations is striking. Despite this, little of it is applied to clinical orthodontics. For this reason, it is not unfair to say, using Dr. Asling's expression, "In our practice we are little beyond the carpenter stage."

This is probably due, at least in part, to the fact that at this stage we do not know where in the upper facial skeleton there is a growth center. Furthermore, as Dr. Crigler has clearly indicated, we seldom see a malocclusion which is solely the direct result of morphologic discrepancies. Indeed, this observation is fully substantiated by our own findings from a representative group of children at ages 6, 8, 10, and 12 years.

This study shows that although the morphology of the face may be significant as a predisposing factor in malocclusion, the maturation process is clearly involved. For example, boys are more likely to have Class II malocclusion at the age of 8 years than are girls, but at a later stage this difference disappears. With this and other studies in mind, I hope that before long we will be able to bring into more active consideration a modifying influence of hormones and nutrition on orthodontics.

Studies on stress lines in the jaws, Dr. Asling mentioned, were carried out by Tappen, who used the same methods as those employed by Benninghoff and Seipel. These observations indicate that the maxilla is not a supporting structure for the other bones in the facial skeleton, nor does it serve as an attachment or buttress for any significant chewing movements.

This would seem to indicate that the maxilla plays a very passive role in the development of occlusion. The real significance of this observation for understanding more about occlusion will become clear in the near future.

The significance of the muscle activity of the masticatory apparatus in establishing an occlusion has certainly been realized and pointed out by Moyers, Pruzansky, and others. Our studies show that the inclination of the occlusal plane, and thus the disposition to malocclusion, varies with the developing dentition. It is due partly to the relationship between the jaws but even more to the chewing pattern and the teeth present in the mouth.

We have heard that conventional myography does not lend itself to clinical studies of forces of mastication, and I am of the opinion that we shall have to develop special instruments and techniques before we are able to study muscle activity patterns and chewing patterns, rather than chewing forces.

Dr. Wylie: Dr. Asling?

Dr. Asling: Two or three questions have been raised here to which I would like to address myself. I started on the studies on cleft palate, and of course this morning's question is the extent to which these animal experiments have any real bearing on our primary interest in the human being.

First, let us consider a specific deficiency which I discussed—pteroylglutamic acid (PGA) deficiency. What is its relationship to human problems? Do we have any implications in etiology? The embryo has a vital need for this vitamin but the mother goes right along perfectly well without having an immediate and constant supply of it, at least in cases of transitory deficiency. Is there an analog for this vitamin which is physiologically inert?

In rats its administration very early in pregnancy results in loss of the pregnancy. This became a matter of considerable interest to the people who were interested in planned parenthood activities, and studies were carried out on the use of this material as a possible means of controlling pregnancy. It was determined that the activity of this antimetabolite was abortifacient rather than contraceptive, if given very, very early in the pregnancy. A few cases are known, however, in which unauthorized and unfortunate undertreatment took place a little too late. People obtained this agent and used it after the first missed period; perhaps they used not quite enough, or they may have waited until the second or third month. At any rate, we now know of two or three cases in which cleft palate occurred as a result of the use of this material as an abortifacient. This does not force us to believe that the mother of everyone born with a cleft palate had a three-day PGA deficiency.

As for the general question of nutritional deficiency, I would say that this is a terribly acute deficiency. It can be managed in three or four days, and it can be managed in a single episode of forty-eight to seventy-two hours. We are not dealing with a chronic malnutrition, at least in this particular etiological factor.

I would almost be afraid, particularly if the press were represented in this room, to remark that one would be a little bit apprehensive about giving a pregnant woman who is vomiting heavily, losing food, fluids, etc., some agents which might suppress bacterial synthesis of this vitamin in the intestine. I do not think it would be well to get into the management of obstetrics at this time.

At any rate, I think that the important point is that we do have experimental maneuvers which dependably and regularly produced cleft palate and which have their comparable circumstances in human beings.

Second, let us consider the mandible. Of the some 600 or 700 cleft palate fetuses that we have studied, I have never seen one in which there was a good, normal mandible. I cannot carry this on to your studies. I would remark on one other matter about the mandible, one apparently confirming matter in this etiology, and that is an instance in which embryonic fluid was removed in early pregnancy in animals. The head was brought down on the torso acutely for several days until the amniotic fluid was regenerated.

When this maneuver was carried on at approximately the critical period for mandibular development and transposition of tongue to palatal process, the young were born with cleft palate, even though there was subsequently an effusion of amniotic fluid and restoration. This is a purely mechanical matter.

There are many other agents, many of them affecting general cartilage and bony formation, which also result in palatal clefts. Unless this issue comes up more, I would like to pass on to the remarks and the queries about neuromuscular reflexes and the relationship to skeletal structures in general. I showed you yesterday a slide of the skull of an animal which had been given heavy doses of growth hormone. I showed you great excrescences on the temporal crest, and I showed you the attachment of the muscle there. We saw these both in the skull preparation and in histologic sections through this region.

As people interested in bone, we are thinking too much about whether a hormone does something for osteoblasts and osteoclasts. I have never seen an animal treated with growth hormone that did not get massively muscular. As this whole discussion takes place this morning, when we are looking at the hormonal effects on bone, we might also be very careful to look at the hormonal effects on the muscle which is pulling on the bone.

I would like to go now to the heel of an animal and remark on some experiments in which the whole cluster of muscle tendons which pass around the heel became enlarged and the tendons became so full of bone that they resembled turkey bones—and you know what the turkey leg is like. Every tendon had a bone in it, and this bone might or might not attach to the heel, and some of the tissue transformations which took place were remarkable. As one tendon passed over the calcaneous and became bony over the calcaneous, a pseudojoint developed to the extent that it had a synovial link and an articulate cartilage on either side. Thus, this tendon provided itself with a joint in order to work very much like the patella on the knee.

Dr. Wylie: Thank you, Dr. Asling. I suspect that there are other comments on this paper, but I shall postpone them. I call now on Dr. Pruzansky to take up Dr. Ralston's paper.

Dr. Pruzansky: You will recall that Dr. Ralston's paper was entitled "Uses and Limitations of Electromyography in the Quantitative Study of Skeletal Muscle Function." He concluded on what might be interpreted as a sour note

when he stated that he "takes a rather pessimistic view of the potentialities of quantitative electromyographic studies in the field of dental research."

This raises two questions: First, we do not know what quantitative electromyography is. What can it do for dental research before we condemn it? Second, are these conclusions entirely valid, or can we remove some of the restrictions or at least comply with the restrictions imposed by Dr. Ralston?

Let me talk first about masticatory efficiency, something that I do not think we talk about enough. Efficiency can be expressed as the ratio of the work done in relation to the energy involved in doing that work. Efficiency of the masticatory mechanism can be subjected to the same kind of analysis. For example, the work output has already been investigated quite thoroughly by Manly and his co-workers, who studied how well certain persons can chew peanuts, etc., but we know very little about the energy requirements during mastication.

It is conceivable that two individuals performing the kind of test that Manly would impose, might triturate with equal ability but one individual would work harder at the job than the other one. It is like saying that when two automobiles are traveling the same speed and going the same distance with the same load, one engine burns up more gas than does the other.

Therefore, a complete estimate of the masticatory mechanism involves a statement regarding not only the work output but the energy output as well. Why is this significant? I think it is particularly significant in the functional evaluation of occlusion. In orthodontics, as indeed in all of dentistry, our analysis of malocclusion is primarily morphologic. The occlusion is either good or bad simply on the basis of various morphologic criteria. While it is true that functional concepts have long been taught and found valuable, there is still no quantitative accurate method for establishing functional norms.

It is conceivable, for example, that if we did have such functional standards they might shake everything that we have been saying on the basis of morphologic evidence. It is quite possible that certain malocclusions might show no functional deficit in terms of mastication, even though they may be morphologically severe.

On the other hand, other malocclusions, some of which might appear too minor to treat, might, from a functional point of view, have rather profound limitations. Now, where does electromyography fit in? Since the action potential from the skeleton muscle is at least some kind of measure of the force exerted by that muscle, it becomes interesting for our purposes to develop an index of what the muscle is doing in chewing.

In his paper Dr. Ralston imposed certain restrictions. He said that one has to define excursion, load, and speed of muscle contraction before one can even begin to talk about quantification.

Can this be shown? I submit to you that it can be done—it has been done. Mastication has been defined by psychologists as a motor automatism; in other words, if you watch a person chewing gum on the subway or on a platform, you will find that he does it rhythmically in almost a metronome-like fashion.

This assumes, of course, that he has a full complement of teeth, or at least that no teeth are missing, for if he has an edentulous space the cud falls into this space and a kind of ataxia develops.

You can use chewing gum; it is not food, but it is a test. Can we define the load imposed by chewing gum? We took every chewing gum that can be bought on the counter and we tested them all. Have you any idea what a bolus of chewing gum weighs and how much water it displaces before you start chewing it, and how much it weighs after, let us say, ten minutes? This is important. If you take bubble gum, it may weigh 3½ grams before you start, and then after you have chewed it for ten minutes it weighs less than 1 gram, indicating that over two-thirds of that chewing gum is soluble. I suspect that most of it is sugar. It displaces one-third of the original volume. These figures are essentially the same for all chewing gums. We went many steps further; we analyzed how long it takes to reach this kind of plateau. We wanted to know how long it takes for this, what can happen when one hydrates this bolus and introduces a lot of saliva. We know all these things. What I am saying is that, to the extent of defining the load, I believe that we are satisfying Dr. Ralston's restrictions. Furthermore, because masticatory excursions in terms of the range of moving for given individuals are almost metronome-like in character over a fairly constant defined range, we can satisfy that restriction, too.

The next problem is to find a way of doing it. We first tried to study all the methods that we could reasonably conceive as being useful in quantifying the masses of muscle. This is purely an empirical study. There are some foundations for this. We tested them all, and we found that method which gave us the most reliable linear response to a changing load. How does this integrator work? You have to calibrate it; you have to put in a certain amount of electricity, and you have to measure pips. These are reconverted to electrical terms; they are calibrated in very precise ways, and they can be calibrated to give a more or less linear response. In other words, as you double the load you double the amount of electrical activity that you are measuring.

This is the kind of record that we got. We had an individual chew first on one side and then on the other; we had him chew one stick of gum and two sticks of gum. Then we made graphs of ipsilateral versus contralateral chewing. We can satisfy the restrictions imposed by Dr. Ralston.

I compliment him on playing the role of Devil's disciple. This is very healthy, particularly in electromyographic research, and in conclusion I would like to join him as a Devil's disciple by pointing out that electromyography is a physiologic tool. It can measure physiologic parameters only. Centric occlusion or, central relation, is not a physiologic parameter; neither is rest position, unless all these things are defined in terms that are very strictly physiologic—and this has not yet been done, at least to my satisfaction.

You cannot take a population of patients and separate them, as this is done in paper after paper, into Angle's Class I, Class II, and Class III and try to find some common physiologic denominator. They do not even have a common morphologic denominator that is completely satisfactory. How can you expect

to find, on a shotgun basis, a physiologic common denominator? I think that better experiments can be designed. To do this one needs the collaboration of people like Dr. Ralston. Thank you.

Dr. Ralston: I may have seemed somewhat hypercritical of the whole matter of quantification. It just so happens, however, that in the work in which I have been involved for about fifteen years a team of men has emphasized the importance of using terms very accurately and not misleading the person who is reading your paper or with whom you are talking. I have had the rare opportunity for all these years of working very closely with a group of very competent engineers on the Berkeley campus and, at the University of California, with an outstanding orthopedic surgeon, with a group of specialists in electronics, etc. After having associated with a group of men like this you begin to be very, very careful of how you talk and what you say, because if you are not careful they are going to jump on you and pretty well tear you apart.

This word "efficiency" which Dr. Pruzansky mentioned has been kicked around for many, many years, and I may say that the physiologists have been as guilty as anybody. In engineering the term "efficiency" means the ratio of the external work to the energy expenditure. The external work in engineering processes or in chemistry is generally something that is readily measured. You lift a load a certain distance, or you compress a gas through a certain volume, and so on. These things are subject to very exact measurement, and the energy expenditure frequently is subject to equally exact measurements. That is the term "efficiency" as the engineer might use it. When you talk about the efficiency of muscle contraction you know neither the work which the muscle does nor the energy expenditure, so you really do not know anything at all about energy expenditure.

Now, if we use these terms in quotation marks and indicate to the reader exactly what we are talking about and what we are trying to do, then nobody is going to object particularly. I would suggest that in studies such as Dr. Pruzansky's that just so long as the author carefully indicates that he is measuring electrical output of a given muscle and is using that as a measure of effort, which is permissible under these restrictive conditions, and so long as he does not stick his neck out too far in talking about quantification but refers to a semiquantitative measure or equilibration of muscle effort, then nobody is going to argue very seriously with him.

Nobody wants to restrict research, and nobody wants to tie chains around a person. I think it is proper, however, to insist that terms be used in their common scientific sense or, if they are not being used in that way, that they be very carefully defined so that the reader is not being misled. I think that the authors of some of these dental electromyographic papers have misled a good many readers because they have talked about energy expenditure when they have had no way of determining the facts.

I certainly agree with Dr. Pruzansky with regard to this matter of centric position, etc. I am not a dentist, but, as I said yesterday, I have been teaching

in dental schools a large part of my graduate life. I have heard these words "centric position" and "centric occlusion" kicked around without ever having the faintest idea of what people are talking about. I do not think I have ever really gotten a first-class, straight-forward definition of these terms. I am reminded of a remark made by a famous early American philosopher who criticized some philosophic work by saying: "Philosophers for twenty-six thousand years have been looking for gold in a mine where there was no gold." This is true in this particular connection. We have to look sharply at our thinking, at our definitions, and at what we are talking about. Thank you.

Dr. Wylie: Dr. Moyers, would you comment on this paper?

Dr. Moyers: I have had some experience in this area, and I agree with some of the things that Dr. Pruzansky said and some of the things that Dr. Ralston said about the problems or differences of quantification in electromyography around the face. The most important thing has not been said yet, namely, that in the case of the facial and jaw muscles we are dealing with several situations that do not obtain anyplace else in the body.

In the first place, we have the feedback mechanism from the teeth, which is highly singular and has a tremendous impact on the way the muscles work. The second factor is emotion. There is some evidence that emotion is more closely related to our facial and jaw muscles than to other muscles in the body. We do express ourselves in our moods and with our facial muscles. This is another factor that ought to be filled in.

I like Dr. Ralston's comments on quantification. Quantification is many things to many people. I would like to submit to both Dr. Pruzansky and Dr. Ralston that each of them has done studies and presented integrators in the literature, as well as diagrams and schemes for integrators. I have noted that quantification means many things to many people, just as integration means many things to many people, both outside physiology and in physiology, and most of the things that are called integrators in the literature are not integrators at all. I certainly second Dr. Ralston's suggestion that the word "quantification" ought to be in quotes.

The last matter that I would like to comment on is centric relation. I tried to explain centric relation in terms of physiologic parameters to a group of prosthodontists, and I suggest that none of you try this. I do not think that prosthodontists know what centric relation is; certainly, it is a difficult thing to define. I am delighted that we had Dr. Ralston on our program, and I am very happy to have heard this discussion of electromyography. It pleases me to find that other people are having difficulties too.

Dr. Wylie: I am going to ask Dr. Moyers to discuss Dr. Crigler's paper.

Dr. Moyers: Even though I am out of my field, I did come to one strong conclusion: I am tired of reading articles and hearing speeches in which people espouse the relationship between systemic disease and malocclusion. I was delighted to hear it said over and over again throughout Dr. Crigler's paper

that there is not any specific or pathologic relationship between disease and a specific malocclusion type. We ought to take that home and never let it affect us semantically or in any other way.

I would like to re-emphasize some of the indices that Dr. Crigler was using. For example, his serologic ideas were much more critical, much more specific, and much more quantifiable than are our indices. Yet, with reference to dental and cranial-facial development (and Garn made that point at the Golden Anniversary Luncheon yesterday) the future might bring one of two things: (1) When we get these specific indices in our area, they may uncover relationships unknown to us, and it is theoretically possible that there might be relationships of which we are now unaware, or (2) the indices might (and I think this is the case) support the thought presented yesterday by Dr. Crigler. It was a pleasure to read the paper.

Dr. Crigler: I do not want anyone to think that I am an authority on dentofacial development either. When I was asked to review systemic disturbances as related to dentofacial development, the only way in which I felt we were at all connected with this was that we had the opportunity to work on our own dental group through our interest in factors involved in general growth, particularly of the skeletal system, since all of us used this as a measure of both an effect of linear growth and an effect of maturation on tissues. The effect of maturation on tissues is not limited to the skeletal system, however, and I have thoroughly enjoyed hearing both Professor Baume and Professor Asling review the experimental work in biology.

I would like to raise a point or two about this or to emphasize a point or two which may apply to clinical medicine and dentistry. I mentioned these briefly a minute ago, but the thing is that one is always interested in the cause of a problem. Here we are speaking primarily of dentofacial imbalance and malocclusion, and if you can get to the cause and prevent the defect, then you have accomplished a great deal.

I was particularly impressed by the very critical timing of nutritional deficiencies in inducing very obvious defects in skeletal formation of the cranium. This is present in other fields, and there are other examples in human biology. We are becoming more and more aware of the importance of any type of therapy or any type of insult occurring during the period of gestation in human beings. It is a very difficult field because, as Dr. Asling pointed out, the nutritional defect or the insult is not necessarily demonstrable in the parent; therefore, your one recourse is take a good gestational history or familial history and come up with a causal factor in relation to the defect.

There are experimental episodes, though, particularly with the administration of hormones during pregnancy which might be mentioned here. These are the progestational growths, most of them synthetic, that induce alterations in the differentiation of genital ducts and external genitals in female offspring. This has been pretty well documented now as a cause-and-effect relationship, and it is also shown in experimental biology.

The interesting thing is the critical period of administration, because if progestational drugs are given after the third month or the second month of pregnancy in the human being no defects are likely to occur, or the defect will be limited to only one portion of the genitals. All of these factors come into our thinking when we talk about skeletal defects, because so often the defect carries over from one system as you are looking at it, particularly to a more generalized defect.

I would like to ask Dr. Asling whether or not the induced lesions he sees—the nutritional lesions—are inheritable. In other words, do these animals grow to maturity and have offspring of their own? Is a chromosomal defect produced in them? Or is this a temporary effect?

Dr. Asling: I cited the work of Hale in 1935 as being the first classic cleft palate experiment. The cross-breeding which Hale conducted in his series of experiments was absolutely admirable, but in no circumstance whatever was he able to carry this forward. You see, one of the problems in a cleft palate kind of experiment is that the young do not do too well. They have to nurse somehow, and if you have given them a badly cleft palate and cleft lip you have a problem in animal nurture, especially in a newborn rat.

In lesser deficiencies we have brought some animals on up to the period of breeding—animals with seven toes, with clubfoot, with cardiac defects—without any transformation. One must modify all this by recognizing that when a genetic predisposition does exist these agents raise the incidence of effect enormously.

Dr. Wylie: Dr. Harvold?

Dr. Harvold: I would like to make a comment on Dr. Crigler's comments. I showed a case to you today in which a woman patient had been using her *pterygoid* muscles for chewing all the time up to the age of 50. She had obviously never used her temporal muscles. At the time she got this reduced equilibration, she started chewing with the temporal muscles more than before, and some change in the mandible occurred. It gave her an open-bite. There was no habit or anything—just a difference in the muscles she used when she was chewing.

This shows that just the way the patient is chewing can alter the inclination of the occlusal plane as much as 8 or 10 degrees with no skeletal changes.

Compare this observation with the observation that we have made in Burlington, where we see that the occlusal plane is changing significantly as the children erupt their teeth, changing to such an extent that it overwhelms the change in skeletal morphology.

With this picture in mind, I am convinced that the future will tell us a lot about nutritional influence on malocclusion (for example, caries and chewing habits directly associated with caries in various areas of the mouth). I do not think in terms of big bones or small bones or strong muscles. Instead, I think of the whole biologic masticatory apparatus, and I am convinced that nutrition plays quite a part in this picture. Thank you.

Dr. Wylie: Dr. Cohen, do you have a comment or two?

Dr. Cohen: If I can focus down a little bit from the general to the more specific aspect of Dr. Crigler's paper, there perhaps may be one or two things that were not brought out yesterday afternoon. We have had the opportunity, for example, to study twenty-five hypothyroid patients from Dr. Crigler's well-stocked human laboratory of endocrinopathies. We examined them rather critically from many angles, and the only significant finding was that in these treated cases the difference between the tip of the central incisor and the "A-P" plane was much greater, perhaps a few millimeters to 6.5 mm. as an average, and that this was one area that did not seem to catch up.

We could speculate on the many reasons. As you know, the defect in the hypothyroid person is more apparent in the midfacial bones, and there are perhaps many reasons and opportunities to speculate, which we do not do at this time. I would like to express our attitude—and I use this word "attitude" advisedly, simply because no other would fit—actively toward the actual treatment of these patients. We think that when these patients come for orthodontic treatment we just roll up our sleeves and go to work and carry out our treatment and diagnosis just the same as we would have without this previous history.

Now this in no way discourages us from continuing further, from trying to get more information to correlate this information to give us more understanding as to what is happening in the face.

Dr. Wylie: Dr. Mathews?

Dr. Mathews: I would like to ask Dr. Crigler to comment on inhibition or stimulation of growth, as the case may be. You have done that somewhat from the hormonal point of view.

Dr. Crigler: I assume that you are asking what things will accelerate and retard growth?

Dr. Mathews: Differentially, and not in a generalized fashion.

Dr. Crigler: Local aspects of the dentofacial bones?

Dr. Mathews: Yes. As orthodontists we are concerned with whether we can stimulate or inhibit the growth of a dentofacial part.

Dr. Crigler: We do not have any adequate quantitative data with which to answer this question, and I would not pretend that anything we presented was of that nature. I hope that some day we will have the opportunity to get it, because these naturally occurring disease states also affect the skeleton of the cranium and the facial bones. Without quantitating it, it is quite easily seen on photographs of these children taken at different times or on lateral films of the skull.

That is the development of the bones, and I would think that if one were careful here and had controlled information concerning the normal range of growth an analysis can go as far as one has the time and energy to take it and that one would see definite maturating effects of not only the hormones but also, perhaps through the hormones, the nutritional effects. One does not have the mechanism by which these things are all accomplished.

I was asking Dr. Asling this morning how he thinks thyroid hormone promotes maturation of the skeleton. We are all quite aware of the fact that it produces it, and he has shown this beautifully. But does this act directly on the bone tissue? Or does it act directly on some other mechanism which then influences the bone?

This is not merely of academic interest, because in physiologic terms there are real differences in growth. In a hypothyroid patient one invariably sees, if the hypothyroidism is of long enough duration (and all these things have a time factor here, which makes this tricky), that persistent skeletal maturation is more markedly delayed than linear growth. I think that, in general, this is shown with experimental animals. On the other hand, in the cases we see of accelerated growth the opposite is true—skeletal maturation is more markedly affected. In the last few years we have been treating our hypothyroid patients and purposely pushing therapy to a limit, because of the central nervous system, to see if statistically we can improve our over-all results. It may not have that effect, but in so doing we have had the opportunity of observing other effects on skeletal growth. As you saw from the cases presented, the human thyroid hormone can promote significant acceleration of skeletal maturation, and it also re-establishes and promotes accelerated growth, but I have a feeling this may be accomplished at a dose level which does not return the metabolic state.

These ranges are difficult to define. If this is the case, is there any probability (since we know that thyroid hormone effects almost all cells and their function) that there is a relationship here between thyroid and adrenal function, that these effects on maturation are perhaps mediated through some other force or other endocrines, or that maybe there is a combination of direct effect on tissue? There are, of course, experimental approaches to this; we have an interest also in rats, and this is being investigated. All of this just points out that you cannot take an intact individual on whom you are working in the study of biology and take an isolated single experiment and draw general conclusions, for there are always many factors that you are not considering.

Dr. Wylie: Dr. Pruzansky?

Dr. Pruzansky: Dr. Asling made an interesting comment regarding the effect of endocrines on soft tissue. Most of us think of acromegaly as being the effect of excess hormone. It is that, but there are other endocrines which become involved. We also associate it with mandibular prognathism, which is not always the early effect but is sometimes a late effect.

Perhaps one of the earliest effects is an enlargement of the tongue. I recall that Cushing showed spacing in the mandibular dentition, and this seemed to be more associated with the tongue becoming larger than anything else. In fact, one of his patients was a preacher who commented about the thickening of his tongue. He said that it made articulate speech difficult. Another feature one notes is the enlargement of the soft tissues of the face and nose and the puffiness of the lips and the cheeks. As you look at a series of these photographs you become impressed that up to a point this mimics aging. I begin to suspect that endocrine physiology may play a role in aging. Look at some family albums

and you can see that as people grow older they begin to take on this broadening of certain tissues and this puffiness, which are almost acromegaly-like changes.

I would like to make a prediction that some orthodontic research in future years is going to shift to the other end of the age spectrum, and, paradoxically, we may become interested in aging in order to understand growth.

Dr. Wylie: Thank you.

Dr. Baume: I would like to comment again on that question by Dr. Mathews about the differential influence on skeletal development. As I said yesterday, in order to understand dentofacial development we have to consider the components, and one of these is the skeletal component. Here we talked about the differentiation between the cartilage bones and the cancellous bones. They are presided over by completely different factors.

There arose the question of muscle physiology. This is extremely important, and I would like only to mention here that muscle has osteogenic properties. If you will look, for instance, at the attachment of the mylohyoid muscle or the temporal muscle you will not find any periosteum. If you cut off that muscle, you also cut off the bone-forming organ. This is one thing that we have to keep in mind. Muscles can even form bone at their attachments, since they contain osteoblasts.

Now if you cut off the muscle, then you may say, "Well, there is no function; there is no bone growth." No, there is no organ that forms bone, and then there is no bone growth." I would like to insist on that fact; it has been overlooked perhaps for too long.

Another tissue is the brain. You have heard that growth hormone does not influence brain growth at all, so again the conclusion that growth occurs from the brain to the brain capsule is not completely valid. There is another organ which we do not know too much about and which we overlook perhaps too much—the tooth germ. We have studied that in our experiments on rats, and we have seen that this dental organ is presided over by completely different principles. In order to get eruption of a tooth we need vitamin A, which is very important, and so we have to see into that.

I also would like to mention the tremendous difference between bone formation and dentine formation. When I looked over the literature on fluoride metabolism I found everything on fluorides, on enamel, and on bone, but not one single word on dentine, or what fluoride does to dentine. There is the early work of Schour, but there is no more analysis, even in human beings. I think these are wide fields into which we must look, and we have to think always in terms of these different influences of the various factors. Only in this way can we finally put together the puzzle and get a full picture. Dentofacial development is certainly a puzzle which is composed of many, many different tissues and many different factors.

Dr. Wylie: Before we close I should like to call on Dr. Salzmann, the program chairman of this meeting.

Dr. Salzmann: By this time you no doubt realize that this meeting is somewhat different from previous meetings. We did not have one orthodontist on the program yesterday. This session this morning is more in the form of a colloquium. It was a discussion of papers presented yesterday, and the discussion was by prepared discussors. It was an attempt not to prove anyone right or wrong but to present existing knowledge on subjects closely related to orthodontics but largely neglected by orthodontists. When I discussed this session with Dr. Wylie, he saw its value and agreed to serve as moderator. Dr. Wylie deserves all the credit for the detailed arrangements, and I wish to thank him and the rest for this splendid and informative session.

As a final word, may I take the opportunity also to thank the essayists who have been very kind to us by coming from their several parts of the country and their various primary interests to give us of their knowledge. I am sure that everyone who spoke here this morning deserves your thanks. Finally, I want to congratulate the audience because you have again shown that orthodontics is more than mere wire bending. Thank you.

EDITORIALS

Protection

WHEN Dr. George Anderson of Baltimore, Maryland, was president of the American Association of Orthodontists in 1959, he requested the editor of the AMERICAN JOURNAL OF ORTHODONTICS to go to press as soon as possible with the current report of the Necrology Committee. That report, published in the January, 1960, issue under the title "Is Your House in Order?" is well worth reading a second time, particularly by dentists who practice orthodontics. At that time the Necrology Committee, under the chairmanship of Dr. William S. Smith of San Francisco, California, expressed its appreciation to President Anderson, William S. Parker, and C. F. Stenson Dillon for their valuable contributions in the past on this subject.

Again in 1961, under the leadership of President Dallas R. McCauley, the A. A. O. membership is being reminded of the importance of the subject of estate planning, particularly as it concerns those in solo practice. The Necrology Committee this year is headed by the man who first alerted orthodontists to a hazard in the practice of their particular specialty. At the request of the officers of the A. A. O. and the Special Necrology Studies Committee, we are glad to publish again Dr. Dillon's original editorial which appeared in the AMERICAN JOURNAL OF ORTHODONTICS in December, 1953.

In addition, members of the A. A. O. may expect printed matter to follow in the mails from both President McCauley and Chairman Dillon.

Editor.

Protect yourself and your interests

ORTHODONTISTS, as a professional group, are improvident, impractical, and unbelievably wasteful. This is not a statement made carelessly or in idle criticism. On the contrary, I make the statement thoughtfully, dispassionately, and without prejudice in the hope of attracting attention to what should be obvious facts. During our lives we work diligently and prudently, and give our almost undivided attention to three aspects of professional pursuit: to find pleasure in rendering competent service to our patients, to live graciously, and to make

adequate provision for our families. It is in this last-mentioned endeavor that we fail so pitifully. We fail, not through intent but through overconfidence, carelessness, and folly. Regardless of the causes, it is true that upon the termination of our lives, we do, for the most part, waste the substance for which we have strived assiduously during our productive years.

This is not an attempt to outline investment procedures, savings plans, or ways of preserving wealth. I am in no position to do that; nor would it be fitting were I to attempt it, even though qualified. My purpose is to bring to light as emphatically as possible the need for all of us to put our houses in order and keep them that way, permanently. Perhaps I am peculiarly well fitted for this task by a series of misfortunes which befell the members of the Pacific Coast Society of Orthodontists during my term as president. In that brief two-year period, death and its tragic sequelae struck with relentless frequency. The Necrology Committee, having no authority and no precedent as to procedure, acknowledged with regret the passing of these men and closed the book on their memberships. This was not enough. As president it became my self-assumed and sorrowful privilege, assisted by friends of the deceased, to act as advisor and buffer between the bereaved, bewildered survivors and the clamorous demands made upon them from all sides. That I was able to help in a small way is to my credit, but for the most part I found myself unable to prevent needless heartache and financial loss resulting from the chaotic condition in which the estates were left. Lest it be supposed that I am exaggerating the importance of an orderly house and the need for the booklet compiled under the thoughful guidance of Dr. Herbert, I shall list some of the situations encountered:

Deceased died intestate.

Last will and testament, if any, not recorded, inaccessible, or location unknown.

No independent bank account for widow or other survivors.

No provision made for monthly withdrawals to sustain relicts.

Court action required to establish subsistence withdrawals.

No record of bank accounts. Pass books to banks not on record.

Office bank account inactivated pending litigation over reimbursement and/or malpractice claims.

Tax records not on file consecutively.

Financial records impounded and removed from premises by Department of Internal Revenue.

No record of investments, titles, or personal property.

No record of insurance policies, including life and malpractice insurance.

Patients' records inaccessible or nonexistent.

Office safe and filing cabinets sealed pending investigation by Department of Internal Revenue.

Safe deposit box number not recorded.

Safe deposit box sealed by Department of Internal Revenue.

Office sealed pending investigation of accounts.

Attorney and court action required to search out assets.

Attorney and court action required to quell suits, institute countersuits, reply to claims, file counterclaims, referee for reimbursement of unfinished, paid-in-full cases.

Delayed and complex probates.

Exaggerated appraisal of estate by court-appointed appraiser.

These are the outstanding obstacles. Each one of them, and there are more, adds to the expense, confusion, and sorrow in the settlement of an estate. Each one exacts a heavy toll of financial, physical, and emotional resources.

Pursuing the effort to find a solution to this poignant problem, I took it to the Boards of Directors of the Pacific Coast Society of Orthodontists and the American Association of Orthodontists. In both instances my pleas were heeded and very competent Necrology Committees went to work. The results of their efforts were submitted to Dr. George Herbert and are now in the hands of each member of the American Association of Orthodontists in the form of a concisely worded booklet containing fifteen pages to be filled out carefully and thoughtfully. *Do not put this booklet aside to be completed at a future date.* Do not assume that your life is interminable and that you have ample time to take care of your responsibilities. Do not assume that the data required to fill the pages of this booklet are inconsequential. Any man who can and will fill out all the pages of this small booklet completely will find himself in a sound position and can feel he has done much to provide protection so that his legatees will not be subjected to harassment from foreseen and unforeseen claimants.

It is not to be supposed that the booklet distributed by the Necrology Committee of the American Association of Orthodontists will solve all of the problems, but it can and will make it possible for each one to institute and execute a practical and helpful plan for orderly estate settlement. From it and its use should come even more tangible and concrete methods for preserving the fruits of our work, our studies, and our lives. Use the booklet and keep it safely in a place other than the office or safe deposit vault. Keep it where it will be accessible at a moment's notice and not subject to impoundment.

My thanks to Dr. Herbert and his committee for putting into usable form the work of many men. Also, my thanks to him and Drs. Pollock and Shepard for inviting me to express my thoughts on this subject.

C. F. Stenson Dillon.

The American Board of Orthodontics

SINCE its founding in 1929, the American Board of Orthodontics has certified 684 orthodontists in forty states, the District of Columbia, the Canal Zone, Canada, and Mexico. While this represents a considerable percentage of the membership of the American Association of Orthodontists, it is far short of

the majority. With the American Dental Association's increasing interest in the regulation of dental specialties, it is to the advantage of orthodontists who have not yet been certified to seek certification.

Directors of the first Board included Albert H. Ketcham, Alfred P. Rogers, Lloyd S. Lourie, B. Frank Gray, Martin Dewey, Abram Hoffman, and Oren Oliver. In the thirty-two years of its existence, the Board has had thirty-eight directors, including some of the outstanding contributors to orthodontic progress.

Certification may soon become more than a status symbol for the orthodontist. A. D. A. regulations may make it necessary for all orthodontic specialists to be certified. Public health agencies, hospitals, and teaching institutions already either demand or favor certification by staff members and chiefs of clinics.

At the twenty-eighth annual meeting of the American Society of Orthodontists, held in Estes Park, Colorado, July 16 to 19, 1929, the American Board of Orthodontia was created under a resolution offered by Dr. Martin Dewey and adopted unanimously.

The authority of the American Board of Orthodontics as a certifying agency is recognized by the American Dental Association through its Council on Dental Education. The agreement between the Board and the Council was concluded on Oct. 27, 1950. The current provisions of the American Dental Association are set forth in a resolution entitled "Requirements for National Certifying Boards for Special Areas of Dental Practice," as adopted by the House of Delegates on Sept. 16, 1959.

Eligibility requirements* are as follows:

1. Active membership in the American Association of Orthodontists for at least two years.

Citizens of other countries, who have been affiliate members of the American Association of Orthodontists for at least two years and who meet all other requirements may apply for "affiliate" certification.

2. Graduation from a dental school accredited or otherwise recognized by the Council on Dental Education of the A. D. A.

3. A license to practice dentistry issued by a legally constituted examining board or other legal authority.

4. An exemplary record of professional ethics.

5. Exclusive practice in orthodontics for at least five years at the time of filing application. In computing this requirement, time spent in full-time training in orthodontics may be included.

6. Minimum training requirements shall be as follows:

(a) Two years' successfully completed graduate or postgraduate orthodontic training in an accredited university school (this means two academic years each of eight months' or more duration); or

(b) Eight months' university graduate or postgraduate orthodontic training in an accredited university school plus (1) twelve months' full-time preceptorship in the office of a member of the American Association of Orthodontists, or by spending sufficient time in hos-

*Changes and modifications may be made in these requirements from time to time.

pitals, clinics, or fundamental science laboratories recognized by the Council on Dental Education of the American Dental Association and by the American Board of Orthodontics as competent to provide an equivalent adequate training in orthodontics, or (2) twenty-four months' supervisory preceptorship as defined below; or

(c) Twenty-four months' full-time preceptorship in the office of a member of the American Association of Orthodontists or its equivalent, including attendance at acceptable special orthodontic courses or in hospitals, clinics, or fundamental science laboratories recognized by the Council on Dental Education of the American Dental Association and by the American Board of Orthodontics as competent to provide an equivalent adequate training in orthodontics; or

(d) Twelve months' full-time preceptorship in the office of a member of the American Association of Orthodontists plus twenty-four months' supervisory preceptorship or an equivalent to the above which may include attendance at acceptable special orthodontic courses or in hospitals, clinics, or fundamental science laboratories recognized by the Council on Dental Education of the American Dental Association and by the American Board of Orthodontics as competent to provide an equivalent adequate training in orthodontics; or

(e) Forty-eight months' supervisory preceptorship or an equivalent which may include attendance at acceptable special orthodontic courses or in hospitals, clinics, or fundamental science laboratories recognized by the Council on Dental Education of the American Dental Association and by the American Board of Orthodontics as competent to provide an equivalent adequate training in orthodontics.

(f) Teaching or a fellowship in the field of the specialty may be considered in partial fulfillment of this requirement.

The American Board of Orthodontics meets once a year, immediately preceding the annual session of the American Association of Orthodontists. At this time, it reviews the applications on hand and approves or rejects them. For each application approved the Board decides upon the requirements for examination and notifies the applicant accordingly. The applicant is expected to have his material for examination prepared by the next annual meeting. If he is unable to complete these requirements by that time, he has the right to petition for an additional year. If this additional year is granted and the applicant is still unable to complete the requirements, he has the privilege of requesting a final year in which to do so.

Those who wish to take the examination can obtain detailed information and guidance by writing to the secretary of the American Board of Orthodontics, Dr. Alton W. Moore, University of Washington, Seattle, Washington, or to any of the current Board members: B. F. Dewel, Evanston, Illinois; Paul V. Reid, Philadelphia, Pennsylvania; Frank P. Bowyer, Knoxville, Tennessee; Nathan G. Gaston, Monroe, Louisiana; Richard M. Railsback, Piedmont, California; and J. A. Salzmann, New York, New York, president of the Board.

The Albert H. Ketcham Memorial Award was established by the American Board of Orthodontics in collaboration with the American Association of Orthodontists in 1936. With the death of Dr. Ketcham on Dec. 6, 1935, the specialty sustained an irreparable loss. As one of the pioneers in orthodontics, he helped blaze the trail into the unexplored realms of its science, doing his work so well that the burdens of those to follow were greatly lightened.

To commemorate his achievements and to serve as an inspiration to the advancement of orthodontics, the American Board of Orthodontics created the Albert H. Ketcham Memorial Award. Recipients of the Ketcham Award include the following:

1937	John Valentine Mershon Philadelphia, Pennsylvania
1938	Alfred Paul Rogers Boston, Massachusetts
1939	Milo Hellman New York, New York
1940	George Wellington Grieve Toronto, Canada
1941	Frederick B. Noyes Chicago, Illinois
1942	Harry E. Kelsey Baltimore, Maryland
1944	B. Holly Broadbent Cleveland, Ohio
1946	Raymond C. Willett Peoria, Illinois
1948	Clinton C. Howard Atlanta, Georgia
1949	William King Gregory New York
1951	Benno E. Lischer St. Louis, Missouri
1952	James D. McCoy Beverly Hills, California
1953	Spencer R. Atkinson Pasadena, California
1954	Charles R. Baker Evanston, Illinois
1955	Joseph E. Johnson Louisville, Kentucky
1956	Leuman M. Waugh New York, New York
1957	Oren A. Oliver Nashville, Tennessee
1958	Joseph D. Eby New York, New York H. C. Pollock, Sr. St. Louis, Missouri
1959	Allan G. Brodie Chicago, Illinois Robert H. W. Strang Bridgeport, Connecticut
1960	Sheldon Friel Dublin, Ireland Charles H. Tweed Tucson, Arizona
1961	William B. Downs Aurora, Illinois

The attitude of the American Board of Orthodontics is one of cooperation with applicants. It is the purpose of the Board to certify as many as possible and to assist applicants in meeting certification requirements. All correspondence is confidential and announcements are confined to those who are certified.

As president of the Board during the 1961-1962 term, it is my pleasant duty to invite every orthodontist to seek certification. Applicants will find the Board ready and willing to assist them in realizing the goal of certification in their specialty.

J. A. Salzmann, president
American Board of Orthodontics

Basic approaches in European and American Orthodontics

FREEDOM of the press, especially the press devoted to scientific matters, is a sacred heritage of American democracy. This holds true for those who agree with us as well as those who are in disagreement. A professional journal, such as the AMERICAN JOURNAL OF ORTHODONTICS, must present diverse opinions and findings. Occasionally some of the published statements, especially those emanating from otherwise reliable sources, call for a reply.

In an article published in this JOURNAL by A. Martin Schwarz,* the following statements are made comparing American with European orthodontists:

> . . . in the American orthodontist's opinion the final form of the whole dentition is, like the shape of the tooth crowns, already definitely determined at birth. Therefore, he is left with a very small field for treatment which involves transforming the alveolar bone artificially by force only. Consequently, he is afraid of disturbing the directed growth by early treatment of the deciduous or mixed dentition during the time of its most impetuous development and prefers to treat the permanent dentition. For the same reason, he is afraid to expand the jaws and doubts the possibility of moving the entire mandible forward out of a Class II occlusion, and so on.
>
> The European orthodontist, on the other hand, believes that he must start treatment during that period of turbulent growth, trying to bring the favorable hereditary tendencies to the fore. He is convinced that this is the best time for successful treatment. He rejects appliances that work only on a tooth and its alveolar bone. Instead, if possible, he uses devices which simultaneously influence all aspects of the malocclusion in two ways: (1) by gentle stimulation to transform the jaws in the direction of the desired hereditary tendency and (2) by radical change of function in the same sense, the function being the best-proved promoter of the hereditary tendencies being sought.

To get the record straight, American orthodontists treat both the deciduous and the mixed dentitions. Many would rather start treatment in the mixed dentition than wait for the malocclusion to establish itself fully in the permanent dentition. Dr. Schwarz is by this time familiar with the widespread use by American orthodontists of extraoral and removable appliances, as well as fixed appliances, in treating deciduous, mixed, and permanent dentitions.

As to expanding the *jaws*, what proof does Dr. Schwarz have that he can expand the body of the mandible? Even the dental arch itself can be expanded only under special circumstances and to a comparatively limited extent. Dental arch expansion, which should have been settled long ago, is still an open question. Expansion of the median palatal suture has been demonstrated. We have yet to learn about the effects of arch expansion on the sutures of the bones at the base of the cranium and the zygomatic processes.

We have pointed out elsewhere that the jaws, the teeth, and the entire stomatognathic system must be regarded as a closed functional system. In order for the dentition to remain in regular alignment after orthodontic therapy, the

*Schwarz, A. Martin: The Task of the Orthodontist Viewed From a Biologic Standpoint, AM. J. ORTHODONTICS 45: 856-860, 1959.

†Salzmann, J. A.: Orthodontics: Practice and Technics, Philadelphia, 1957, J. B. Lippincott Company, pp. 471-472.

teeth must be in consonance with the rest of the closed stomatognathic functional system, which includes the jaws, the muscles of mastication, and the mimetic muscles. The components of the stomatognathic system are not free agents of which each can be permanently changed at the will of the orthodontist without affecting the stability of the rest of the components.

The teeth can be moved with success orthodontically within an area of tolerance where they are not in conflict with the trajectories of force of the closed functional stomatognathic system. Orthodontic movement of incisors lingually or labially can bring them into conflict with the closed functional system of the stomatognathic complex and beyond the area of tolerance where these teeth will remain stationary. When teeth are moved outside the area of tolerance, relapse follows. The limits of the area of tolerance, as far as the individual patient is concerned, are as yet unknown. It will probably be established in the future when electromyographic analysis is perfected. Until that time, expansion of dental arches will remain an unknown quantity and can be judged in retrospect only.

J. A. S.

DEPARTMENT OF ABSTRACTS AND REVIEWS

Edited by DR. J. A. SALZMANN, *New York City*

All communications concerning further information about abstracted material and the acceptance of articles or books for consideration in this department should be addressed to Dr. J. A. Salzmann, 654 Madison Avenue, New York City.

Serial Models Illustrating Some Spontaneous Changes in the Deciduous Dentition

By B. C. Leighton D. Practitioner **11**: *109-112, November, 1960*

Much has been written in support of the thesis that the pattern of development of the masticatory apparatus is determined at an early age. This is by no means the case with occlusion. A certain degree of flexibility is permitted in the early stages by adjustments to the path of closure of the mandible and by migration of individual teeth within the alveolar processes.

Chapman reported that, although the distal surfaces of the upper and lower second deciduous molars were frequently in the same vertical plane before the age of 3 years, this relationship may be improved by forward movement of the lower molars relative to their opponents by the age of 6 years. The change was said to be associated with a greater increase in upper arch width and a decrease in the incisor overbite, which was considered by Friel to be necessary to facilitate this movement. More recently Bonnar, in a serial study of sixty children, found that in some cases spontaneous improvement of a postnormal occlusion may occur at the time of eruption of the second deciduous molars, the first permanent molars, or the permanent incisors.

In a series of 212 cases it was found that occlusion deteriorated as frequently as it improved. In forty cases some alteration of occlusion was exhibited between the third and fourth years. The incidence of changes which may be equally for better or for worse suggests that they are not usually part of a developmental pattern but, rather, are chance manifestations. Little evidence could be found to indicate that the changes were associated with sucking habits, swallowing behavior, or the relationship of soft tissues to the teeth.

In eighteen of the forty cases the second deciduous molars were found to have been in occlusion at the age of 2½ years, that is, before the changes of occlusion occurred. A random one in five sample of the author's cases was examined; of forty-two cases twenty-one were found to have had the second deciduous molars in occlusion by the age of 2½ years. It would appear, therefore, that there is not always a close association between changes in occlusion and eruption of the second deciduous molars. In most cases, however, some evidence was found of adjustments of mandibular position when the teeth were put into occlusion. These adjustments were manifested as alterations in the relationship of the center lines of the two arches.

It is suggested that the changes in relationship which take place spontaneously are usually readjustments of an uncomfortable occlusion. In some cases

this may be stimulated by the eruption of the second deciduous molars and subsequent closure of molar spacing. It appears that environmental factors may bring about these changes only occasionally.

Correlation Between Malocclusion, Oral Habits, and Socio-economic Level of Preschool Children

By Louis J. P. Calisti, M. Michael Cohen, and Martha H. Fales. J. D. Res. **39**: *450-454, May-June, 1960.*

Four hundred ninety-one preschool children were examined by the same examiner, with the children's parents present. The orthodontic examination consisted of (1) observation of the opening and occluding of the dentition; (2) observation of the teeth in a static centric occlusion to determine classification; (3) observation of the patient for any obvious habits; (4) examination of the patient's hands, lips, and tongue for evidence of habits; and (5) questioning of parent and child as to known habits which might influence the dental structures. Notation was also made of the presence or absence of spaces between the anterior teeth of the deciduous dentition.

Although Angle's classification of malocclusion is admittedly not designed for the deciduous dentition and is incomplete in its description of all Class I malocclusions, it nevertheless was used in this study because of its efficiency in a survey of this type and its frequent use in the dental literature.

The following descriptions were used to denote tooth position when the maxillary and mandibular teeth were in contact and in centric occlusion: anterior open-bite, premaxillary protrusion, deep overbite, end-on (edge-to-edge), and lingual posterior cross-bite.

The habits considered in this study were finger-sucking and similar hand pressures, fingernail biting, tongue habits, and lip or cheek habits.

Distribution of malocclusion according to Angle's classification shows that 89.5 per cent of the cases fell into Class I. This compares favorably with 89 per cent found in a similar survey of 443 Brookline preschool children. In relating malocculsion to the three socio-economic groups, there was no significant relationship between socio-economic level and malocclusion.

A significant relationship was found between habit and malocclusion. There was a definite trend in the percentage distribution of habits as related to socio-economic levels. Why habits should be significantly more frequent at higher socio-economic levels is a subject for future investigation.

Baume found anterior spacing in 70 per cent of the cases that he studied. The present survey showed open spaces in 142 cases (30 per cent).

On the basis of findings that children in higher socio-economic groups have significantly more oral habits than those of the middle and lower socio-economic groups and that habits are significantly associated with malocclusion, one should not be disturbed by the fact that there is no significant association of high socio-economic groups with malocclusion. A trend in malocclusion from high to low socio-economic groups does exist, but, since a positive correlation was

not demonstrated, it is not surprising to find the lack of significant correlation between malocclusion and socio-economic level. However, the significant relationship of habit to malocclusion in each socio-economic group confirms the hypothesis that habit is related to malocclusion.

A Cross-Sectional Study in Dental Development and Skeletal Age

By Aubrey M. Lauterstein J. Am. Dent. A. **62**: *161-166, February, 1961.*

One hundred thirty-two white children, ranging in age from 61 months to 82 months, were studied to investigate the relationship between root age, bone age, and number of erupted permanent teeth. Intraoral, hand, and wrist roentgenograms were taken of each child, and at the same visit the number of permanent teeth present in the child's mouth was counted.

Bone age, root age, and number of teeth present were recorded for each child. Rank order correlations were determined from these data.

The study demonstrated the following:

1. Those children with advanced root age also had advanced bone age. Conversely, those with a comparatively younger root age had a younger bone age.
2. The number of permanent teeth erupted in the child's mouth did not bear a high correlation to the child's bone age.
3. The child's root age increased as the number of permanent teeth erupted in the mouth increased.
4. Chronologic age bore a positive correlation to number of erupted permanent teeth (0.378), root age (0.324) and bone age (0.292). These correlations, however, were considerably lower than the significantly higher correlations of root age to bone age (0.437) and root age to number of teeth (0.426).
5. The probabilities of getting the correlations observed by mere chance were less than 1 in 100 under the null hypothesis.
6. It now appears reasonable to hypothesize that a longitudinal, sex-specific, cephalometric roentgenographic study of bone age, root age, and number of erupted teeth would contribute significantly to the creation of an atlas of dental development that could be clinically useful in the prediction of eruption time of teeth in boys and girls, the treatment of dentofacial disharmonies, and the adoption of root age as a criterion of the developmental status of the child.

The Relation of the Temporal Muscle to the Form of the Coronoid Process

By Virginia Avis. Am. J. Phys. Anthropol. **17**: *99-104, June, 1959.*

The reasons for the dependence of bone upon muscle are open to question. Some attribute this dependence largely to mechanical factors, for example, tension exerted by the muscle on the growing bone. Others prefer to regard bone growth as the result of vascularization.

This experiment was designed to alter the mechanical situation, while keeping all other factors normal, by removing a large amount of the temporal

muscle but leaving all that immediately adjacent to the coronoid process. In this way it was hoped to separate the results of muscle tension from the results of vascularity of the bone. The split-line method of Benninghof was used, and cats served as the experimental animals.

The temporal muscle on the operated side had not regenerated, and it extended only slightly above the tip of the coronoid process. The portion of the muscle which remained arose from the zygomatic arch and infratemporal fossa of the skull and inserted into the coronoid process.

At the dorsal border of the muscle lay the temporal line, bounding the infratemporal fossa, far below its position on the normal side. The parietal bone was reduced in thickness and lacked some of its normal curvature, so that this aspect of the skull appeared flatter than did the opposite side. While the nuchal crest was well below normal in size, the interparietal bone had suffered an even greater reduction in growth.

Gross examination showed no abnormality of the dental arch, and this impression was confirmed by the measurements taken after maceration. There was no differential wear of the teeth. The only difference between the two sides of the mandible lay in the coronoid process. The process on the unoperated side was similar to that of normal cats, but the coronoid process on the operated side, while only slightly smaller than its fellow, differed markedly from it in shape. In vertical dimensions the altered process was somewhat shorter than the normal one, but the major change was seen to be the shift in its axis. Instead of describing a curve upward and posteriorly, as in the normal condition, the coronoid process of the operated side of the jaw rose straight upward, lacking entirely any suggestion of the normal curve.

This modification in external form of the coronoid process was reflected also in the split-line pattern. On both of its surfaces a coronoid process is characterized by a pattern composed of two components—anterior and posterior. In the normal bone these converge near the base of the process and continue posterodorsally in the curved axis of the bone, ending at its posterodorsal border. In the altered coronoid process, the posterior component was greatly reduced in extent and the enlarged anterior division deviated from its normal orientation halfway through its course to become anterodorsally directed and to end adjacent to the anterodorsal border.

Removal of a large part of the temporal muscle, but not that surrounding the coronoid process, results in a process that not only is somewhat smaller than normal but also has an external conformation and internal structure that are notably deviant. Despite this, the remainder of the mandible is completely normal in all respects.

This supports the concept of the mandible as a composite of relatively independent parts, a suggestion made by Washburn in 1951 and elaborated by Murphy in 1957. Among an increasing number of workers, this concept, extended to the rest of the body, has largely replaced the older view whereby the body was regarded as a unit whose parts had all evolved in unison and a single feature might thereby serve as a valid index of taxonomic status.

An interrelationship does exist between certain of the features. It is also certain that such a series of interrelated features forms a pattern which underlies and makes possible the successful performance of a particular activity which forms an integral part of a way of life.

The removal of all the temporal muscle dorsal to the zygomatic arch effected two major changes: (1) The strength of the muscle was reduced, and (2) the direction of tension was changed. Lack of abnormality in the dental arch, together with the normal chewing pattern of the animals, showed that the muscle was a functioning one with adequate vascularity. Clinical work has shown that immobilization or nonuse of any part of the body in the normal individual results in the partial or complete failure of osteoblasts (in the affected region) to secrete the organic intercellular substance which is calcified to form new bone. For this reason, some workers believe that "stress and strain stimulate osteoblastic activity." The smaller coronoid process in the present work is easily interpretable in terms of lessened tension of the muscle and its effect upon osteogenesis.

Whether the observed changes are produced by a "normal pattern of growth acting on a substrate of altered form" or by "mechanical adaptation," the fact remains that formation of the internal structure has shifted in conformity to the extrinsic forces acting upon it.

Results such as these lend support to Tappen's conclusion that those changes which occur in the split-line pattern of the juvenile gorilla skull in the transition to the adult condition are in response to altered mechanical conditions.

Recent developments not only make it possible to evaluate the morphogenic powers of muscle tissue but suggest the mechanisms by which many of the external prominences of cortical bone are, as Washburn suggested, maintained by their muscular attachments.

No longer is there reason to doubt the active role which mechanical forces (for example, muscle tissue) play in many aspects of skeletal morphology, a topic upon which so much stress has been placed in phylogenetic studies. While it is undoubtedly true that other processes are of considerable importance in bone formation, recognition of bone-muscle relationships makes it possible to place evolutionary studies upon a much firmer basis than heretofore.

Effect of Hand-Mouth Contacting on Neonatal Movement

By Joanna P. Williams and William Kessen. Child Development **32**: *243-248, June, 1961.*

Contact between the hand and the mouth in the infant has received considerable theoretical attention. The great frequency of occurrence of hand-mouth contact in infants and the continuation of the behavior in the finger-sucking of older children suggests that it is a response of some importance. The fact that this response is intimately related to vital consummatory acts suggests the possibility that its elicitation or execution may involve some rewarding, relaxing, or "satisfying" effects.

Hoffer has argued that the effectiveness of hand-mouth contact may be due to the fact that it produces a reduction in "tension," a theoretical construct related to "drive," as used by Hull, or "libido", as developed by Freud. Some of the behavior patterns found by Wolff in his systematic observations of newborn infants indicate that such a theoretical formulation may be fruitful.

The relationship between hand-mouth contacting and activity, measured by a film analysis technique, was studied in an investigation of the hypothesis that hand-mouth contact is a tension-reducing activity in the newborn. The present study utilized filmed observations of the first, third, and fifth days of the lying-in period of ten newborn infants.

No significant differences were found between the mean activity level of the five seconds preceding a hand-mouth contact and that of the five-second period immediately following such a contact. Regression of activity on time preceding a contact was not significantly different from zero. There was a significant but very small decrease (opposite in direction to expectation) in activity as time following a hand-mouth contact increased. When the data for the first day were eliminated from the analysis, none of the relations was significant; nor were the results of an analysis of the first day's data alone.

The implication and generality of these findings were discussed.

The Number of Hand-Wrist Centers

By Stanley M. Garn and Christabel G. Rohmann. Am. J. Phys. Anthropol. **18**: *293-299, December, 1960.*

The numerous ossification centers of the hand and wrist that appear during postnatal life offer a potential opportunity for an objective, quantitative measure of developmental status. A count of hand centers, expressed perhaps as a percentage of the maximum, affords the simplest measure of osseous development without need for pictorial reference standards.

Variability at different ages must be taken into account. When variability is small, it is difficult to establish gradations of osseous status. Moreover, intercorrelations between successive ages must be determined on an individual basis. If children differ widely in status at different ages, a simple enumeration of hand-wrist centers has obvious limitations.

While the hand does contribute twenty-eight centers out of the total of sixty used for evaluation, it is not certain whether the hand is representative of the remainder of the appendicular skeleton. Definitive data have been lacking.

The present study was concerned with the number of hand-wrist ossification centers present at various ages during infancy and childhood, the extent to which hand-wrist status at one age is related to hand-wrist status at another age, and the degree to which the hand is in agreement with the remainder of the appendicular skeleton during infancy and childhood. Recognizing the technical simplicity of a posteroanterior hand film and the saving in radiation which it affords, the central problem is whether a simple enumeration of centers can serve as a continuing index of individual developmental progress.

An interrelationship does exist between certain of the features. It is also certain that such a series of interrelated features forms a pattern which underlies and makes possible the successful performance of a particular activity which forms an integral part of a way of life.

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The numerous ossification centers of the hand and wrist that appear during postnatal life offer a potential opportunity for an objective, quantitative measure of developmental status. A count of hand centers, expressed perhaps as a percentage of the maximum, affords the simplest measure of osseous development without need for pictorial reference standards.

Variability at different ages must be taken into account. When variability is small, it is difficult to establish gradations of osseous status. Moreover, intercorrelations between successive ages must be determined on an individual basis. If children differ widely in status at different ages, a simple enumeration of hand-wrist centers has obvious limitations.

While the hand does contribute twenty-eight centers out of the total of sixty used for evaluation, it is not certain whether the hand is representative of the remainder of the appendicular skeleton. Definitive data have been lacking.

The present study was concerned with the number of hand-wrist ossification centers present at various ages during infancy and childhood, the extent to which hand-wrist status at one age is related to hand-wrist status at another age, and the degree to which the hand is in agreement with the remainder of the appendicular skeleton during infancy and childhood. Recognizing the technical simplicity of a posteroanterior hand film and the saving in radiation which it affords, the central problem is whether a simple enumeration of centers can serve as a continuing index of individual developmental progress.

The number of hand-wrist ossification centers present at one age is not a generally useful measure of developmental status for individual normal children. Though distinctly applicable on a group basis and for children beyond the fifth and ninety-fifth percentile limits, predictability from one age level to another is insufficiently high to commend a simple "carpal count" as a precise tool for serial growth evaluation.

After the third of fourth year variability in the number of centers becomes restricted, so that a simple enumeration is of value for only a limited time during infancy except, of course, in cases of endocrinopathies, infantile malnutrition, and growth failures.

The number of postnatal ossification centers in the hand and wrist was investigated in 154 Ohio-born white subjects at twenty-three successive age intervals from 1 month through 10 years.

The number of centers at a given age was, at best, moderately correlated with the number of centers present at another age, with an average r of 0.40 over a four year interval.

Hand-wrist ossification counts did not prove adequately representative of the remainder of the appendicular skeleton at ages 2.0 through 6.0.

The hand-plus-foot combination, though it accounted for a maximum of forty-eight of the sixty postnatal ossification centers, yielded no marked improvement in predictability at 4.0 years of age.

It was concluded that the hand-wrist ossification count, though obviously useful in studies of growth abnormalities and of grouped data, is not a precise measure of developmental progress in normal children.

New book ready

A new book, *Transactions of the Research Workshop on Roentgenographic Cephalometrics*, edited by J. A. Salzmann, is now available. The book may be ordered from the J. B. Lippincott Company, 5 East Washington Square, Philadelphia, Pennsylvania.

NEWS AND NOTES

American Association of Orthodontists 1962 Research Meeting

The research meeting of the American Association of Orthodontists will be a program consisting of a series of ten-minute research reports which shall be presented orally or read by title only. All persons engaged in research in orthodontics or cognate fields are invited to participate in this program, which will be held April 29 to May 3, 1962, in Los Angeles, California.

Each participant is asked to prepare a 300 word abstract of his research project for publication in the AMERICAN JOURNAL OF ORTHODONTICS and a 25 word summary of the work to be included in the program for the meeting. Abstracts for publication, summaries for the program, and the ten-minute oral presentations to be given at the meeting should be carefully prepared in order that an adequate description of the import of the work may be presented.

Forms for use in submitting the titles, 300 word abstracts, and 25 word summaries of research projects will be sent to the orthodontic department of each dental school and to any individual requesting one.

In order to be included in the program of the research meeting in Los Angeles, titles, abstracts, and summaries of research projects must be mailed not later than Jan. 1, 1962, to Dr. Richard A. Riedel, Department of Orthodontics, University of Washington, School of Dentistry, Seattle 5, Washington.

Faustin N. Weber, Chairman, Research Committee
American Association of Orthodontists
Professor and Head, Department of Orthodontics
University of Tennessee
847 Monroe Ave.
Memphis 3, Tennessee.

American Board of Orthodontics

The next meeting of the American Board of Orthodontics will be held at the Statler Hotel in Los Angeles, California, Monday through Saturday, April 23 to 28, 1962. Orthodontists who desire to be certified by the Board may obtain application blanks from the secretary, Alton W. Moore, University of Washington, School of Dentistry, Seattle 5, Washington.

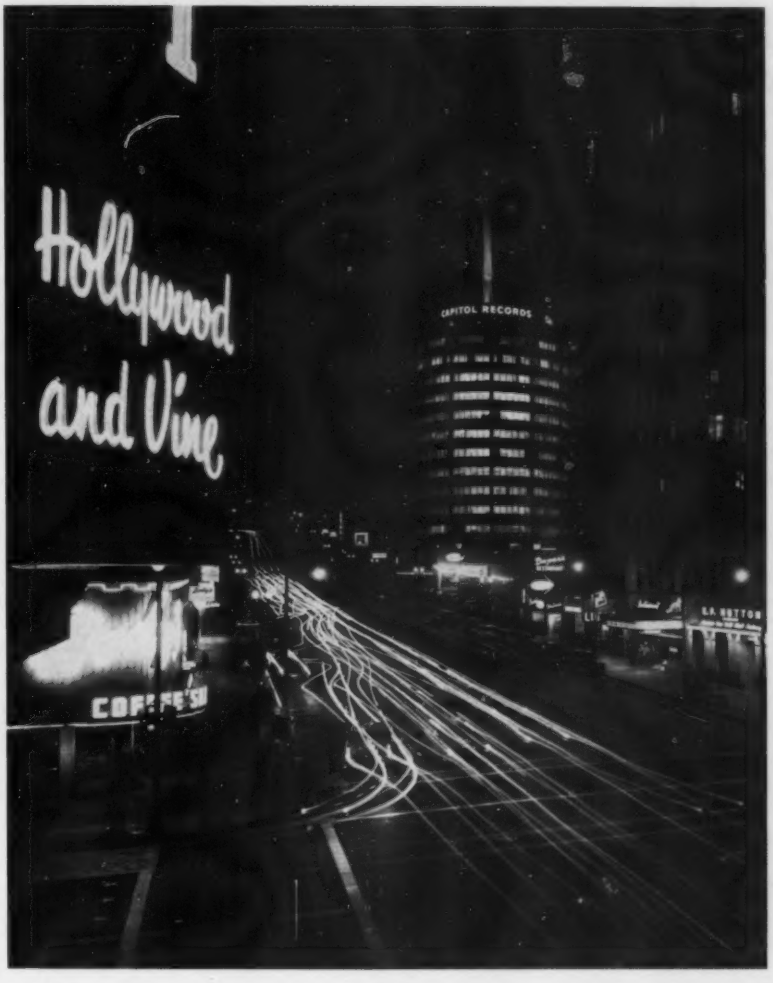
Applications for acceptance at the Los Angeles meeting, leading to stipulation of examination requirements for the following year, must be filed before March 1, 1962. To be eligible, an applicant must have been an *active* member of the American Association of Orthodontists for at least two years.

Komo mai nou ka hale! Those who plan to attend the American Association of Orthodontists meeting in Los Angeles are reminded to mark their calendars also for a thrilling postconvention meeting in Hawaii on May 4, 1962. Highlights of this visit to our fiftieth state include the following:

Pan American Super Jet round-trip flight from Los Angeles to Honolulu.
Ocean-view rooms at the beautiful and famous Hilton Hawaiian Village.
Traditional *aloha* greeting and farewell with flowered leis.
Cruise of historic Pearl Harbor.
Real Hawaiian native feast luau at Don the Beachcomber's.
All-day sightseeing on the Island of Oahu.
Mt. Tantalus tour with lunch at Waioli Tearoom Grass Shack.
Moonlight dinner and sail on the famous *California Barkentine.*
Aloha cocktail party in the Tapa Room at Hawaiian Village.

American Association of Orthodontists 1962 Milo Hellman Prize Essay Contest

ELIGIBILITY. Any member of the American Association of Orthodontists and any person affiliated with a recognized institution in the field of dentistry or associated with it as a teacher, researcher, undergraduate, or graduate student shall be eligible to enter the competition.



One of the most famous street corners in the world, this is the way the corner of Hollywood Boulevard and Vine Street looks to the visitor. Note the round skyscraper in the background. Within walking distance are movie, and television, and radio studios and the famous Chinese Theater where visitors match their own feet in footprints of the stars cast into cement of the forecourt. (Courtesy All-Year Club of Southern California.)

CHARACTER OF ESSAY. Each essay submitted must represent an original investigation and contain some new significant material of value to the art and science of orthodontics, and it must be the contestant's first research orthodontic publication.

AWARDS. Two Hellman Awards are offered—one for the best work presented (the experienced as well as the new researcher is eligible for this award) and one for a researcher presenting his initial essay. A cash prize of $500.00 is offered for the essay judged to be the best submitted from those papers that represent the contestants' first research essay. However, the Committee reserves the right to omit either or both awards if, in its judgment, none of the essays is considered worthy of an award. Honorable mention will be awarded to the four authors whose essays are ranked immediately below the prize essay. Each of the honorable mention awards carries a $100.00 cash prize. As in the case of the prize essay, the Committee may, at its discretion, omit any or all honorable mention awards if the entries submitted are not worthy of them. The Hellman Award essays and the first two essays

meriting honorable mention will become the property of the American Association of Orthodontists and will be published. All other essays will be returned.

SPECIFICATIONS. All essays must be in English, typewritten on 8½ by 11 inch white paper, double spaced, with at least 1 inch margins. Each sheet must be numbered and bound or assembled with paper fasteners in a "brief cover" for easy handling. The title of the essay should appear on the cover. Five complete copies of each essay, including all illustrations, tables, and bibliography, must be submitted. The name and address of the author must not appear in the essay. For purposes of identification, the title of the essay and the author's name, together with a brief biographical sketch which sets forth his or her dental and/or orthodontic training, present activity, and status (practitioner, teacher, student, research worker, etc.), should be typed on a separate sheet of paper and enclosed in a plain sealed envelope. The title of the essay should appear on the outside of this sealed envelope.

PRESENTATION. The author of the winning essay will be invited to present it at the meeting of the American Association of Orthodontists in Los Angeles, California, April 29 to May 3, 1962.

JUDGES. The entries will be judged by the Research Committee of the American Association of Orthodontists.

FINAL SUBMISSION DATE. No essay will be considered for this competition unless it is postmarked on or before Jan. 1, 1962, and five copies of the essay sent to Dr. Albert P. Westfall, University of Texas Dental Branch, Department of Orthodontics, Houston 25, Texas.

Faustin N. Weber, Chairman, Research Committee
American Association of Orthodontists
Professor and Head, Department of Orthodontics
University of Tennessee
847 Monroe Ave.
Memphis 3, Tennessee.

*American Dental Association**

ADA STAFF RECEIVES DENTAL COVERAGE

ADA employees and their dependents will be covered by dental insurance starting Jan. 1, 1962, Sec. Harold Hillenbrand has announced. The insurance program is similar to the pioneering plan established by Continental Casualty Co. in 1959 for the employees of Dentists' Supply Company, York, Pa., and to the plan established in 1960 for Kerr Manufacturing Company, Detroit. The ADA program will also be operated by the Continental Casualty Company.

Under the Association program, all types of dental services are covered with the exception of purely cosmetic dentistry or services covered by other health insurance. Any licensed dentist may be chosen, and he may charge his customary fee; there is no fee schedule or table of allowances. The ADA will pay the entire annual premium. Employees will share in the cost of dental care, paying from 20 to 40 per cent, depending on the type of dental service needed.

BANK BEGINS GROUP DENTAL CARE PLAN

The Mechanics Bank of Richmond, Calif., has begun a group dental care plan for its employees which is being underwritten by the California Dental Association Service. The plan began operation July 1. Dr. Lyall O. Bishop, president of the Service, said that some 700 dentists will cooperate in putting the plan into effect. Coverage includes a wide scope of dental restoration and maintenance with such specific items as prophylaxis, oral surgery and endodontic care.

*Excerpts from the *ADA News Letter,* Sept. 1, 1961.

Great Lakes Society of Orthodontists

It has become necessary to change the dates of the annual meeting of the Great Lakes Society. The meeting will be held Nov. 26 to Dec. 1, 1961, at the Eden Roc Hotel in Miami Beach, Florida.

The following papers will be presented:

Radiographic Interpretation of Temporomandibular Joint Disturbances. William Updegrave, Philadelphia, Pennsylvania.

Diagnostic Procedures for Disorders of the Temporomandibular Joint. Laszlo Schwartz, New York, New York.

Harmonious and Disharmonious Functions of the Temporomandibular Joint's Musculature and Occlusion. John R. Thompson, Chicago, Illinois.

Techniques for Radiographic Surveys of the Temporomandibular Joint. William Updegrave, Philadelphia, Pennsylvania. (Dr. Updegrave will supplement his paper with a table clinic.)

Methods of Treatment of Disorders of the Temporomandibular Joint. Laszlo Schwartz, New York, New York.

The Interrelations of Facial Pattern, Facial Growth, and Function. John R. Thompson, Chicago, Illinois.

Mandibular Ostectomy and Osteotomy. Robert Ponitz, Ann Arbor, Michigan.

Light Wire and Light Forces. Parts I and II. Charles Burstone, Indianapolis, Indiana. (Dr. Burstone will supplement his paper with a table clinic.)

Treatment in the Mixed Dentition. Charles Tweed, Tucson, Arizona.

Treatment in the Adult Dentition. Charles Tweed, Tucson, Arizona.

Cephalometric Analysis for Comprehensive Diagnosis and Special Treatment. Scott Holmes, Muskegon, Michigan.

Other highlights of the meeting will be the presentation of American Board case reports, a symposium by Drs. Updegrave, Schwartz, and Thompson, table clinics, a review of institutional research from 1954 to 1960 by Ben Williams of Columbus, Ohio, business sessions, and installation of new officers.

Social activities will include special luncheons, a fashion show for the ladies, two night club tours, and the president's reception and banquet.

Northeastern Society of Orthodontists

The fall meeting of the Northeastern Society of Orthodontists will be held Oct. 29 to 31, 1961, at the Hotel Manger in Rochester, New York. This meeting is rather special, since it will mark the fortieth anniversary of the founding of the Northeastern Society. An appropriate banquet to celebrate this occasion has been scheduled for Monday evening, October 30, in the Grand Ballroom of the Hotel Manger.

The Executive Committee, under the capable chairmanship of Nicholas Ippolito, has prepared a very worth-while scientific program. Among those scheduled to appear as essayists, are such outstanding teachers and clinicians as Edward Martinek, Jacob Subtelny, Frank Lamons, Morris Stoner, Lawrence McKiver, David Ast, and William Biederman. In addition, the Local Arrangements Committee, under the able chairmanship of Barnett Frank, has assembled a splendid grouop of table clinicians who also will cover a wide range of timely subjects.

This will the first time in the forty-year history of the Northeastern Society that it will convene in the beautiful city of Rochester. The ladies are being encouraged to attend. In addition to the fortieth anniversary dinner dance, a tour of Kodak Park has been arranged

for the ladies, with a luncheon provided by Eastman Kodak Company. The Northeastern Society is also providing a hospitality room, so that the ladies may be guests of the Society at a continental breakfast. While a great deal of effort has been expended in preparing an outstanding scientific program, the social aspects of the meeting have also been given considerable attention.

During one phase of the meeting, the Rochester Dental Society is planning to honor Leuman Waugh for his outstanding contributions to our specialty. This is particularly appropriate, since Dr. Waugh spent his boyhood in Rochester and it was here that he began his career in dentistry. We in the Northeastern Society of Orthodontists feel honored that Dr. Waugh has been singled out for such recognition.

Southern Society of Orthodontists

The next meeting of the Southern Society of Orthodontists will be held Nov. 5 to 8, 1961, in St. Petersburg, Florida. A digest of the program follows.

Sunday, November 5

Registration and recreation.

Monday, November 6

Case Analysis and Treatment Planning. William L. Wilson.
Practical Aspects of Diagnosis in Orthodontics. J. A. Salzmann.
Tooth Position—The Incidental Result of the Reflex Forces of the Dento-Oro-Facial Complex. William A. Buhner.
Functional and Appliance Dynamics in Class II Cases. William L. Wilson.
Principles in Treatment of Class II Malocclusion. J. A. Salzmann.

Tuesday, November 7

Appliance Design and Use in "Borderline Cases." William L. Wilson.
The Responsible Person. J. Wallace Hamilton.
Luncheon honoring new members. (Speaker: Dallas R. McCauley, president of the American Association of Orthodontists.)
Table Clinics (Ed Baker, Chairman).

Wednesday, November 8

What We Know and What We Do in Orthodontics. J. A. Salzmann.
Case Reports. Tyler Haynes, William Ditto, and R. B. Smythe.
Problems and Pitfalls in Orthodontics. William L. Wilson.
Question-and-Answer Period.

First international conference on oral surgery

The American Society of Oral Surgeons has extended a cordial invitation to all members of the medical and dental professions throughout the world to participate in the first International Conference on Oral Surgery, which will be held July 1 to 4, 1962, in London, England.

Southwestern Society of Orthodontists

The next meeting of the Southwestern Society of Orthodontists will be held at the Sheraton-Dallas Hotel in Dallas, Texas, Oct. 22 to 25, 1961. The program follows:

Sunday, October 22

Registration and recreation.

Monday, October 23

What Is Light Force and Why? Arnold E. Stoller.
Identification of Tongue and Facial Problems. Joseph G. Schurter.
Control of Light Force. Arnold E. Stoller.

Tuesday, October 24

Causes of Neuromuscular Problems and When to Treat Them. Joseph G. Schurter.
Practical Application of Light Force Therapy. Arnold E. Stoller.
Training Program. Joseph G. Schurter.

Wednesday Morning, October 25

Table clinics.

Courses in orthodontics sponsored by universities

The following postgraduate courses will be given at the University of Michigan during the coming school year:

Application of Theoretical Mechanics to Orthodontics. C. J. Burstone.
Light Differential Forces in Orthodontic Treatment. J. R. Jarabak.
Occlusal Adjustment and Retention Problems in Orthodontic Treatment. R. E. Moyers and S. P. Ramfjord.
Practice Administration for the Orthodontist. C. E. Martinek and Staff.

Forthcoming meetings of the American Association of Orthodontists:

1962—Statler Hotel, Los Angeles, California, April 28 to May 3.
1963—Hotel Fontainebleau, Miami Beach, Florida, May 5 to 9.
1964—Palmer House, Chicago, Illinois, May 10 to 14.
1965—Dallas Statler-Hilton, Dallas, Texas, April 25 to 30.

*Officers of Orthodontic Societies**

The *American Journal of* ORTHODONTICS is the official publication of the American Association of Orthodontists and its component societies. The Editorial Board of the JOURNAL is composed of a representative of each of the component societies.

American Association of Orthodontists *Next meeting April 28–May 3, 1962, Los Angeles*

President, Dallas R. McCauley, 410 South Beverly Dr., Beverly Hills, Calif.
President-Elect, J. A. Salzmann, 654 Madison Ave., New York, N. Y.
Vice-President, J. Lyndon Carman, 501 Republic Bldg., Denver, Colo.
Secretary-Treasurer, Earl E. Shepard, 225 South Meramec, Clayton, Mo.

Central Section of the American Association of Orthodontists *Next meeting Oct. 1-3, 1961, Minneapolis*

President, Henry E. Colby, 1850 Medical Arts Bldg., Minneapolis, Minn.
Secretary-Treasurer, Kenneth E. Holland, 1016 Sharp Bldg., Lincoln, Neb.
Director, G. Hewett Williams, 811 Elm St., Winnetka, Ill.

Great Lakes Society of Orthodontists *Next meeting Nov. 26–Dec. 1, 1961, Miami Beach*

President, Paul V. Ponitz, 914 Security Bank Bldg., Battle Creek, Mich.
Secretary, Edward A. Cheney, 2900 Grand River, Lansing, Mich.
Director, Harlow L. Shehan, 601 Jackson City Bank Bldg., Jackson, Mich.

Middle Atlantic Society of Orthodontists *Next meeting Oct. 1-3, 1961, Atlantic City*

President, Paul V. Reid, 1501 Medical Arts Bldg., Philadelphia, Pa.
Secretary-Treasurer, Charles S. Jonas, Mayfair Apts., Atlantic City, N. J.
Director, Louis E. Yerkes, 825 Linden Ave., Allentown, Pa.

Northeastern Society of Orthodontists *Next meeting Oct. 29-31, 1961, Rochester, N. Y.*

President, Irving Grenadier, 888 Grand Concourse, New York, N. Y.
Secretary-Treasurer, David Mossberg, 36 Central Park S., New York, N. Y.
Director, Norman J. Hillyer, 230 Hilton Ave., Hempstead, L. I., N. Y.

Pacific Coast Society of Orthodontists *Next meeting February, 1964, Las Vegas*

President, Herbert V. Muchnic, 435 N. Roxbury Dr., Beverly Hills, Calif.
Secretary-Treasurer, Warren A. Kitchen, 2037 Irving St., San Francisco, Calif.
Director, William S. Smith, 2530 Bissell Ave., Richmond, Calif.

Rocky Mountain Society of Orthodontists

President, Louis J. Williams, 9th & Center Sts., Casper, Wyo.
Secretary-Treasurer, Hubert J. Bell, Jr., 230 Mercantile Bank Bldg., Boulder, Colo.
Director, Louis J. Williams, 9th & Center Sts., Casper, Wyo.

Southern Society of Orthodontists *Next meeting Nov. 5-8, 1961, St. Petersburg*

President, Charles E. Harrison, 362 Sixth St., S., St. Petersburg, Fla.
Secretary-Treasurer, William H. Oliver, 1915 Broadway, Nashville, Tenn.
Director, Boyd W. Tarpley, 2118 Fourteenth Ave., S., Birmingham, Ala.

Southwestern Society of Orthodontists *Next meeting Oct. 22-25, 1961, Dallas*

President, Bibb Ballard, 7713 Inwood Rd., Dallas, Texas
Secretary-Treasurer, Tom M. Matthews, 8215 Westchester Dr., Dallas, Texas
Director, Nathan Gaston, 701 Walnut St., Monroe, La.

American Board of Orthodontics *Next meeting April 23-28, 1962, Los Angeles*

President, J. A. Salzmann, 654 Madison Ave., New York, N. Y.
Vice-President, B. F. Dewel, 708 Church St., Evanston, Ill.
Secretary, Alton W. Moore, University of Washington School of Dentistry, Seattle, Wash.
Treasurer, Paul V. Reid, 1501 Medical Arts Bldg., Philadelphia, Pa.
Director, Frank P. Bowyer, 608 Medical Arts Bldg., Knoxville, Tenn.
Director, Nathan G. Gaston, 701 Walnut St., Monroe, La.
Director, Richard M. Railsback, 1333 Grand Ave., Piedmont, Calif.

*In order to keep this list up to date, the editor depends on the various sectional editors to notify him immediately of changes in officer personnel.

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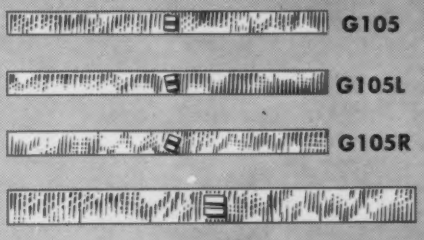

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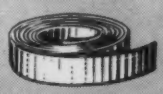

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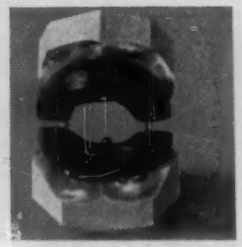

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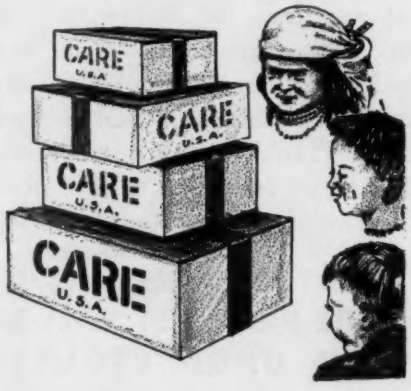

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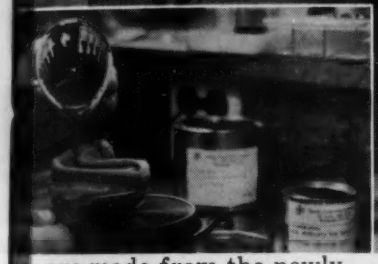

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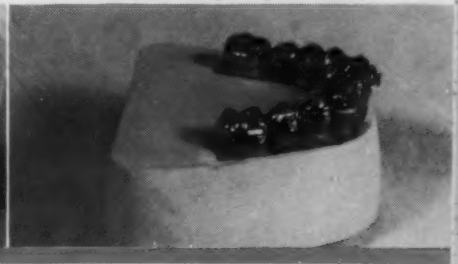

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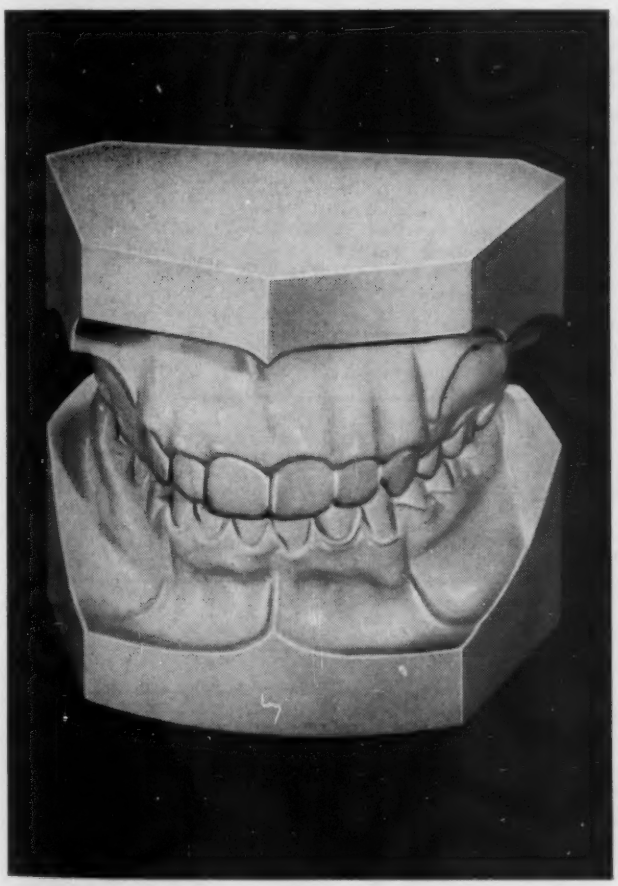

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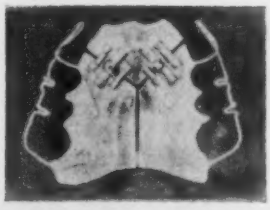

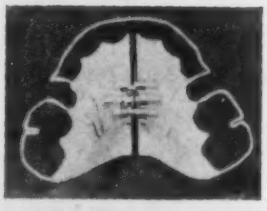

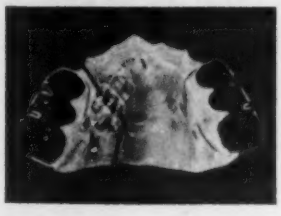

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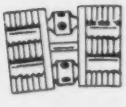

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